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CONTENTS

Bacteriology in General Education: PROFESSOR VERANUS A. MOORE 277

The Address of the President of the Paleontological Society: DR. JOHN M. CLARKE .. 284

The Distribution of the College Men 296

Scientific Notes and News 296

University and Educational News 299

Discussion and Correspondence:—

Botanical Evidence of Coastal Subsidence: PROFESSOR D. W. JOHNSON. *Miastor and Embryology:* DR. E. P. FELT. *Trematodes of the Dry Tortugas:* PROFESSOR EDWIN LINTON 300

Quotations:—

Commercialism in Education 303

Scientific Books:—

Leduc's Théorie physico-chimique de la vie et générations spontanées: PROFESSOR BASHFORD DEAN. *Kisskalt and Hartmann's Prakticum der Bakteriologie und Protozoologie:* PROFESSOR CHARLES A. KOFOID 304

Triphenylmethyl: PROFESSOR J. BISHOP TINGLE 306

Special Articles:—

Suggestions as to the Culture of Butterflies: PROFESSOR JOHN H. GEBOULD. *Variegation of European Alfalfas:* DR. L. R. WALDRON 307

Societies and Academies:—

The Geological Society of Washington: EDSON BASTIN and FRANÇOIS E. MATTHES 312

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BACTERIOLOGY IN GENERAL EDUCATION[1]

IN casting about for a topic around which might be gathered a few thoughts suitable for this occasion, I was impressed with the lack of appreciation of the value of bacteriology in general education and the comparatively small amount of systematic effort that has been put forth to establish efficient methods for teaching this important new science. I became so deeply interested in these phases of the subject that perhaps with more zeal than wisdom I chose for my topic "Bacteriology in General Education."

The practical value of any branch of science keeps pace with the general knowledge of it and an understanding of the methods by which it can be applied. This in turn depends largely upon the teaching of those who are versed in its details of fact and law. The earlier recognition of bacteria did not suggest the important rôle they play in nature's economy. While their existence had been known for more than two centuries, it was not until their causal relation to infectious diseases was recorded that they made an impression upon society in general, and the medical profession in particular. It was the discovery of this great power which they possess to destroy man and beast that afforded a point of contact between humanity and this vast, invisible, organic world about us. If I correctly understand the meaning of our declaration in the constitution of this society, that its object is the promotion of the science of bacteriology, it would seem

[1] Presidential address delivered before the Society of American Bacteriologists, Ithaca, N. Y., December 28, 1910.

that our first and most sacred duty is to ascertain the facts in the life history of microorganisms, to describe the laws governing them and to bring this knowledge into bold relief, in order that so far as possible it may be utilized for the benefit of humanity.

It is often the tendency of men who spend their lives in the seclusion of scientific research to become less mindful of the possible practical application of their work than they are of the technical details and of the truth which it reveals. This is of necessity a natural tendency, for application can not go in advance of the discovery of the facts to be applied. History is replete with illustrations of unsuccessful efforts to benefit humanity by applying theory or dogma in the place of knowledge which perhaps at the time did not exist. On the other hand, the results of investigations of the men of pure science have often led to the formulation of exceedingly practical procedures. In the development of the theory and of the application of a science, therefore, we have to deal with two factors, namely, the intellectual longing for truth regardless of its immediate significance, and the desire to apply specific knowledge for the benefit of society. Occasionally these two elements are equally marked in the same individual. As an example of a mind dominated by the experimental method, as well as the desire to alleviate the suffering and to improve the life conditions of mankind, there is no name that can be mentioned with greater fitness than that of Louis Pasteur, the founder of bacteriology as a practical science.

Almost from its inception, bacteriology has been a technical subject with important practical applications. It has been the agent that has revolutionized the medical professions, vegetable pathology, dairying and soil economics. The bacteriological work that is being done at the present time is for the most part aimed directly at some one or other of the problems in these practical fields. There are few sciences in which the newly discovered facts or laws have been so quickly utilized for some real or hoped-for benefit to man or beast, as in microbiology. The result of this haste to apply new theories has tended to confuse the public relative to the possibilities of a knowledge of microorganisms. It is difficult to have the truth crystallized that bacteriology is a technical subject which can be understood by the specialist only. The tendency to immediately apply newly cited facts, before they are properly demonstrated, has not infrequently permitted errors to be proclaimed as truth, with ultimate disappointment for both those looking for beneficial results and those attempting to attain them by the use of methods based on somewhat hypothetical data.

This is illustrated by the present confusion relative to the various groups of bacteria. The discovery of the tubercle bacillus was followed by the declaration that it was the same in all mammalian tuberculosis. This assumption was applied in the prophylactics of tuberculosis before the important fact was ascertained that there are varieties or races of tubercle bacilli, and also that there exists a large group of bacteria morphologically and micro-chemically at least related to them. Laymen and even medical men are having difficulty to reconcile more recent findings with the original teaching relative to the identity of mammalian tubercle bacteria. I do not refer to this disparagingly, for progress is sometimes made by retreats as well as by advances, but could the truth concerning the tubercle group of bacteria have been determined before the application of the hypothesis that the etiological

factors of human and bovine tuberculosis were identical, much controversy would have been avoided and numerous sacrifices and hardships would have been saved. I have called attention to but one of the important subjects in which the earlier positive statements have been greatly modified by later discoveries, for the purpose of emphasizing both the difficulties and the responsibilities of the bacteriologist in developing a pure science and in applying it in a practical manner.

From the earliest times, life has been given the highest price in the rôle of earthly possessions. How to prolong life has been the theme of many. In more recent years, economists have considered at length the cost to the state resulting from immature death and for maintenance of its sick and diseased citizens. The country is spending millions of dollars annually to support hospitals and asylums for the incurables. As Messenger has stated it, such a work is greatly to the credit of our hearts but not so much to the credit of our heads. Society has been temporizing with these great vital and economic questions, for the aim of the future is to restrict the need for hospitals by preventing disease. The nation-wide conservation movement has a large task to prevent the damages wrought in nature's economy by microorganisms. Again the state and society are calling upon the bacteriologists to bring to the pressing needs of to-day a knowledge of the vital forces that will tend to purify polluted streams and make the soil more fertile. The prevention of infectious diseases and the conservation of the soil are two great tasks that confront practical bacteriologists. The question is: How, in the present demand for applied bacteriology, can this science attain to its highest development and at the same time render the greatest service? The declaration in our constitution limits our field to the promotion of the science of bacteriology, while other organizations have largely for their duty its utilization. It is practically impossible to make this differentiation complete, for as yet there are no societies that seem to deal with the application of important recent bacteriological findings. It is clear, however, that the essential purpose of this organization is to safeguard the purity of the science and to point out the way for its advancement.

The fact has already been conceded that bacteriology is the science that has much to do, and in certain instances practically all to do, in finding the solution and in settling the details of many of the common every-day problems in the conservation of life and health of the higher forms of life. Yet this science, with such possibilities, possesses scarcely any interpreters of its actual value to society at large, and it finds little or no place in the curriculums of our schools and colleges for general education.

The true significance of a knowledge of bacteriology should require no explanation. Because it has grown up through technical laboratory research and routine, educators seem to feel that its true service is restricted to such laboratories. For the highly specialized or professional work we agree, but should not the knowledge of basic facts and natural laws that have transformed so many practises in medicine, sanitary science and agriculture become common property? Is there not a place in our common schools and liberal arts colleges for courses in bacteriology for the purpose of imparting fundamental knowledge that will enable society to come into possession of an understanding of this science? It can not be successfully gainsaid that information concerning the cause of fermentations, the storing of nitrogen in the soil, the causes for the changes in food stuffs

and the etiology of the common infectious diseases is as important, or the acquisition of such knowledge of as much disciplinary value, as the study of the life history of the denizens of the deep as now required in many if not most biological courses. The world looked with wonderment at the hitherto unparalleled success in avoiding preventable diseases by the Japanese army at the time of the Russo-Japanese war. Up to this conflict it is affirmed that for every man killed with a bullet in war four died from infection, while in the Japanese army for every man who died from infection four were killed with bullets. The explanation of this victory over all the experience of the past was found to be the training of the Japanese soldiers in the principles of bacteriology. It should be clearly understood that this marvelous success was due simply to the application of knowledge which Europe and America had brought forth and which the Japanese had applied. Would it not be quite as valuable in civil life to have the people versed in the simple facts and principles of a science that would enable them intelligently and designedly to avoid infection, strengthen the efforts of the sanitarian, and enable the agriculturist to restore fertility to the soil, as to have them possessed of a knowledge of things far remote from their immediate environment? It is not my desire to belittle any course of instruction, for all knowledge is valuable, but I do wish to record the opinion that a science like bacteriology, which plays such an important part in the immediate physical well-being of every individual, should have a place in the curriculum of the schools for the masses.

Because bacteriology is a technical subject with numerous avenues of usefulness there is the greater need, in order to prevent errors in its adaptation, for efficient instruction concerning it in both popular and technical schools. The prevention of its false application will attain its maximum success after the diffusion of knowledge concerning microorganisms. Knowledge is power, and when the people are versed in the fundamental principles of this science they will avail themselves of such information to improve their condition economically and to avoid the dangers of infection.

The position occupied by bacteriology in general education will depend largely upon its teachers. At the present time it has not been granted, with few exceptions, a place in the curriculums of popular educational institutions. Sanitarians and others have endeavored to educate the people in its principles by imparting isolated facts. This is having a certain beneficial effect, but unfortunately the recipients are unable to adapt this fragmentary knowledge to new conditions. At present the masses depend for their guidance in this subject upon the results obtained in professional laboratories and the teaching in technical schools. As bacteriology had its origin in the findings of those seeking the cause of important phenomena, it is to be expected that, for a time at least, its fountain head will continue to be in the results forthcoming from such researches. As these investigators are largely its teachers, we are brought to a discussion of methods for teaching it in professional schools.

I am led to touch upon this subject from the point of view of students, as I have known them, who have received their elementary training in bacteriology in different places. Every one who has to deal with post-graduates in this subject appreciates fully the difficulties encountered in properly orienting the subject-matter taught to beginners in order to produce the desired

result. It is the presence of these difficulties that prompted the opening of this subject, which can not be closed until after the results of many trials and careful discussions are recorded. On my shelf are numerous text-books and laboratory manuals for teaching this science. If they are carefully analyzed, they will give a variety of impressions as to the purpose of the course prescribed. Here then is found the logical reason for the discussion of the fundamental principle in its teaching. The crux of the question is: Shall bacteriology be taught as a science in our professional schools or shall simply the essential facts to be applied be imparted?

Some years ago I accepted a position carrying with it the responsibility of teaching bacteriology in a professional course. I went from a research laboratory where I had grown up with the technique and knowledge of a certain phase of the subject. I labored under the delusion that the essential elements could be easily taught. There was no difficulty in securing the interest of the students, but the pangs of disappointment were mine when these same interested men attempted to apply the knowledge I supposed they possessed, in the actual practise of their profession. Their errors were not more grievous than those of other men, but the things they did and the kind of assistance they sought pointed clearly to a lack of knowledge or understanding of the subject which I believed I had taught with great clearness. This combined experience has caused me to question the efficiency of many pedagogical methods employed in teaching bacteriology, and to test as best I could different methods of instruction. The conclusion that seems to me inevitable is that the course in bacteriology must be dominated by a scientific system of presentation and that the technique, facts and laws of bacteriology must be clearly developed before too much stress can be placed upon the value of fragmentary facts such as staining tubercle bacilli, or the examination of water for the colon bacillus. The difficulty does not rest alone in a lack of the knowledge of technique in making the examinations, but quite as much in the inability to interpret the findings. Conclusions drawn from isolated facts by partially informed individuals are responsible for many of the charges of error against our laboratories. There is, therefore, no way to more effectively promote this science than to have its conclusions based on accurate and verified data.

I have considered the difficulties encountered by way of lack of time granted to bacteriology in curriculums, the eagerness of many inexperienced men to assume charge of bacteriological work and the pittance of salaries often paid for this service. These are recognized hindrances, but they are far more easily overcome than the results of hurried application, snap diagnosis, indifference and commercialism that dominate this work wherever the spirit of science and regard for scientific verification are not enthroned. If the public is to be benefited, as it should be, by the existence of this science, it is important that those responsible for the training of men to occupy positions in public laboratories take fully into account the nature of their teaching. Wherever the true scientific spirit dominates the final results prove to be more helpful. It is not my purpose, nor do I believe it possible, to outline a schedule to be followed by all. It is, however, within our power to give to bacteriology the dignity commensurate with its vital relations to our environment and to teach it as a science and not as an aggregation of biological facts.

I can not leave this subject without

pointing to the possibility of misguided application of this science because of a lack of understanding of it. The mere knowledge of the presence of microorganisms in milk, in water and in dust is responsible for many unprofitable propaganda. In following these, the real dangers may be overlooked. This is illustrated in certain practises. There are, for example, those who, because of a low bacterial count of milk, consider it of excellent quality, without taking into account the condition of the udders or the general health of the cows from which it was obtained. In certain lines of domestic science much stress is placed on the bacterial content of dust and the unsanitary condition their presence creates, when the actual problem is simply one of cleanliness, as the organisms present are of less significance than the dust itself. Regulations based on certain microbian findings and the importance thereof are frequently appealed from because of the seemingly unwarranted expenditures they impose. Many of these interpretations prove later to be without justification, and the annoyance and perhaps hardships they occasioned would have been avoided if the science of bacteriology rather than a few isolated facts had been applied.

There is at present a great demand for men who are capable and qualified to bring to the aid of those struggling with great sanitary and economic problems the relief that is possible, or that seems to be attainable by the utilization of the laws of microbiology. The question we have to consider is how this demand can be satisfactorily met. How under the present restrictions of bacteriology in technical schools and the willingness—because of the lack of knowledge of it—to keep it out of curriculums of schools of liberal arts and sciences, are we going to secure the men and train them to efficiently occupy these positions? The answer may come that our problem does not differ from that existing in other biological and physical sciences. This may be true, but the fact remains that we are a body organized to promote this science and consequently clothed with responsibility that we can not throw off relative to the ways and means to be employed in bringing into existence more efficient bacteriology. The lesson taught by the older branches of knowledge, such as chemistry and physics, points to but one solution, namely, the upbuilding of the purity of the science.

Thus far, bacteriology has operated as an aid to many branches of applied learning, like medicine and agriculture, and for that reason it has not reached its full development as an independent science. Because of this its greatest value has not been secured by those activities which it has served. Its application has been so immediate that most of those who have come in possession of any knowledge of it have been attracted to some field for its application. Some of these places require more breadth of knowledge than others, but there are indeed few of them where bacteriology is dignified as a pure science and its truths and laws studied, tested and cherished as such. The difficulties met in nomenclature, bacterial floras and technique speak for themselves concerning the fragmentary status of the knowledge of this subject. With its diversion to practical lines there is too much specialization before there is a foundation of basic knowledge sufficient to support the superstructure. The desire for application rather than the scientific spirit is manifested too early and often too conspicuously in the devotees of this subject.

Perhaps the greatest difficulty in finding men desirous of taking up bacteriology for a life work is the fact that it is an un-

known topic to them until they reach it in some technical or professional course. By this time their mind is usually centered on the line of attainment desired, and the hurried instruction they receive in bacteriology does not illuminate the science sufficiently to attract them. To overcome this, two alternatives present themselves, namely, to have a preliminary course in bacteriology in the preparatory school or to have the course in the professional schools extended and taught more efficiently. It seems to me that it is a mistake to assume that persons who have received instruction in some narrow field of the subject, and who may decide to follow this work, are qualified to take at once positions of responsibility. There is no other science where accuracy, correct interpretation and application are of more vital importance than they are in this, yet there is no other science where men are elevated to positions of responsibility with so little real preparation. We seem to have forgotten that in the acquisition of knowledge and the coming into an understanding of bacteriology the element of time and the discipline of routine work and research are as essential as they are in chemistry or in physics. Again, in the preparation we must not be unmindful of the necessity of a thorough preliminary training in the fundamental sciences and modern languages.

The question is a perplexing one. The errors of insufficiently trained workers call forth trying criticisms that those who are responsible for laboratory work must bear. Yet when more thoroughly qualified men are sought for they can not be found. What then are we to do? We each and all who are in professional schools must feel that the regular course or courses we give in bacteriology can not fully or adequately prepare one for teaching the subject or taking a responsible position in it. Yet the student has ability, is earnest and a hard worker. He is financially so situated at the time of his graduation that he can not continue his studies. In this condition the acceptance of the proffered position, with the mental resolution that he will study hard and later do graduate work, is apparently the thing for him to do. The outcome of such a course is well known. The numerous and unexpected difficulties attending the new position together with the increased amount of routine exhaust the energies and the hoped-for special training is rarely secured and the desired efficiency is not attained. Until we have a better system for preparing young men the present practise will continue and our science, our laboratories and the public must suffer the consequences.

I have dwelt somewhat at length on the difficulties which exist in the promotion of the science of bacteriology. If they are analyzed it will be found that they are temporary, and incident to the way by which man found his dependence upon his microbian environment. After the numerous demands for men to do work, which the recognition of the importance of microbes has created, bacteriology will undoubtedly develop into a pure science that will be as eagerly pursued as any branch of learning in the acquisition of an efficient education.

In conclusion, I desire to make a few suggestions as to the possible means for both promoting this science as such and increasing its value as an asset in practical knowledge.

The first is to introduce in the courses of biology in our common schools, instruction on the existence and varieties of bacteria, and something of the action of a few of the more important economic species. This would afford a little fundamental knowledge to help the masses to better understand the cause of many natural phenomena.

The second is that there should be established in our normal schools, colleges and universities thoroughly scientific courses in bacteriology. Such a course would lay the foundation of the science and place it on a par with other branches of biology, such as botany and zoology. Such departments should be prepared to do research, thereby fitting men for the special phases of the subject in technical and professional schools, and qualifying others to do research of a purely scientific nature, without reference to any particular trade, occupation or profession. The problems here are as numerous and their solutions are as taxing upon the mental forces of the investigator as they are in any branch of scientific endeavor.

The third suggestion is a plea for more scientific methods of teaching bacteriology in our professional schools. The course should, as a rule, be lengthened and the theoretical teaching supplemented by as much practical work as possible.

We should in view of all conditions rejoice in the achievements of the first half century of practical bacteriology. There is every reason to believe that with a better understanding of the vital relation of the microbian environment to higher forms of life bacteriology will be productive of still greater benefits, because of the education of the public concerning it and because of its more intelligent application by all.

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THE PALEONTOLOGICAL SOCIETY[1]
ADDRESS OF THE PRESIDENT

There is a respectable virtue in the observance of well-tried usages. In the societies out of which we have emerged it has been the established procedure to penalize a retiring president with a somewhat formal address. It is a practise which is at once a solace and an opportunity; the former, inasmuch as successors to this honor of office must share these heart-searching efforts of its closing hours; an opportunity, for here is an outlet to unspoken cogitations which seldom take on the formal expression of the printed page, a chance to weave together the threads of evidence or suggestion we may have followed many years and left dangling; perhaps even, to perfect into some well-finished form the summation of our larger problems. It seems to have been tacitly assumed by our council and membership that I should inaugurate for this society the accustomed procedure anew. I frankly face this situation, but with no promise of the adequacy of the outcome.

I. At the outset I propose to take brief advantage of my own somewhat peculiar position and experience as a public official in paleontological science as a point of view from which but few of you, my audience, may have had opportunity to contemplate the subject. You will, I pray, be indulgent with this exploitation of a personal attitude, for it does seem to have certain complementary attributes which may in a way illuminate and supplement your own experience. If you find it a rather frank expression it will be couched in terms of fraternal regard, and I hope entirely devoid of unfair or invidious comment which might expose the writer to the charge of being a profane railer at the sanctities. My contact with the public of an intelligent and progressive government is now of rather long standing and this experience has been illuminating in any estimate of public sympathy with pure science.

[1] Delivered by request at a joint meeting of this society and the Geological Society of America, Pittsburgh meeting, 1910.

It is safe to say that most of you are doing your work in the atmosphere of academic vales and cloisters where the prosecution of pure science is an elemental virtue. The very air you breathe is an inspiration to pursue any esoteric cult to any limit, howsoever far it may shape its course away from any apparent direct bearing on human happiness. An enlargement of human knowledge, even though already enlarged beyond human power to grasp, and the possibility of enlarging the comfort and hopes of mankind are justification for this pursuit, if justification be called for. Happily it is not needed, for the very next step, the scientific fact that lies unturned at our feet or the discovery just round the corner, may be as essential to philosophy and to progress as all that have passed by in their countless array. At your command is all that is best in the sciences and among your academic fellows in towns well oxygenated by the very air you exhale, you attain to the full measure of useful citizens. It might be trite for me to say that glorified with a splendid growth in the conservatory atmosphere of the university, the professor of science strays abroad from penetralia of his divinities with an acquired confidence of superiority, into a hardier air and a coarser grained world which declines to take him at his own valuation. I am disposed to think the sheltered aisles of great federal bureaus afford a very like protection from the assaults of the so-called practical community.

But take a man of pure science, and stand him out in the open where he must come into daily contact with men of affairs, men who are making a noise and doing that part of the world's work which feeds upon publicity, with the pulverizing wheels of political machinery and with the commercial sentiment which dominates to-day more than ever before nearly every phase of government and nearly every aspect of society, and if he fails to take his own measure of so-called "practical" usefulness to his community of city or state, it is taken for him. If he can stand the shadowless glare of such an attitude he realizes at length that he is not indeed as other people are. By devotion to the duty his science imposes he may merit the confidence of his sponsors, but he is not likely to become the director of a bank or be invited to sit as a member of important fiduciary or social trusts. He is, in plain truth, quite generally regarded in the community as a rather unproductive if not visionary member of society with a large supply of useless knowledge but with a very limited capacity for making dollars, not always immaculate in his attire or particularly well groomed in person; somewhat terrifying socially, though harmless, perhaps, in his honest effort to turn a rather unintelligible hobby into the means of a livelihood. This expression is not, I think, an unfair phrasing of the general public attitude in our communities toward, not the man of pure science alone, but all high scholastic attainment. Notwithstanding the intellectual regeneration beginning with the last half of the last century which has tended to place all lines of human thought on a scientific basis, our best communities are still largely thinking deductively, and the rest entirely so whenever their thoughts have time to reach beyond their income; and we too may indeed confess freely and without shame even in this presence that something more of the prophet's vision, of the philosopher's method and of the poet's inspiration is stealing back into our own intellectual modes. You and I, men who are giving our best years and skilled energies to the elucidation of the organic law, the higher

law which alike makes the flower bloom and the philosopher think, the one law which governs all the world save only human society, are reasonably content in our security that we have chosen the good part and we decline to subject our standards to those of the eminently companionable and high-minded citizenry of the communities in which it has pleased God to place us.

Paleontology is, in my oft-asserted conviction, the most far-reaching of all the sciences. In it lies the root of all truth, out of it must come the solution of the complex enigmas of human society. Whatever it may be in the first instance, it is in the event that greatest study of mankind—man. It is the panoramic display of the life of the ages, the expression of the organic law of a hundred successive worlds. To quote the expression of Depéret: it has won its independence and is now marching hand in hand with biology toward the discovery of the history of the development of living beings and the laws which control their transformations. We unbar its doors and unlock its secrets only with the key of the present. To me it seems idle to fatuity to magnify the supposed imperfections in the record of life on the earths that have passed. Such efforts are constantly recurring, and when taken out of their proper setting are the last resort of the theorist and the special pleader. The known fauna and flora of the living earth is a percentage of the actual fauna and flora, and only in less degree is the known life of the past since Azoic time to the actual life of that past. The argument here has never been adequately presented, the facts never fairly marshalled, and we all too easily in our self confidence are wont to forget that after only a few brief years of study of the earth we still are standing at the threshold of the earth's history and with a few facts in our possession grow impatient to habilitate the earth, while its deeper chambers still conceal records reserved for our successors to uncover. In the long ages which lie before the human race must come through greater wealth of knowledge a clearer view and more fruitful application of the truth we seek. To-day we begin to see the broad generalizations of the biologic law as based on paleontology which twenty years ago we believed so secure and illuminating, slowly yielding under the weight of accumulating evidence, and we should realize more deeply than ever and should, I believe, endeavor to leave the impress on our successors that it is still more knowledge that we require—it is still the facts we want, rather than the apparent inferences from those we have. Were demonstration of the propriety of this attitude required, let me venture to cite, as an interested onlooker, the enormous volume of unpublished data bearing on the life of Cambrian time which has been brought together by the secretary of the Smithsonian, facts which when known and assimilated must modify nearly every published conception of the development of life on the early earth. A few years ago at the opening of this century I ventured to address a personal letter to some ten men recognized as European leaders in geological work and thought, asking an expression as to what, in their judgment, would be the problems before the geologists of the coming generation. The replies were all alike in essence and reducible to this: The acquisition of more facts. To-day we see only through a glass darkly into our cathedral where the truth lies enshrined.

We know well enough, indeed it is a healthy sign that we are growing painfully conscious, that the standards of our best civilization are not those which have been

derived from a clear apprehension of the paramount law. We are certainly justified in our impatience with these standards so far as such impatience helps to make them or mankind better. Whoever seriously contemplates the historical development of our culture standards out of a barbaric past can hardly fail of restiveness under them, whether of our educational system with its burden of time-worn incongruities and maladaptation to highly differentiated individualities, in which to the common scandal of the race our youth have been treated as papers of pins, machine-made watches, or as communities of ants; of our various religions "with their mossy heritage of half truth and half fiction," of commerce with its Midas dreams and cut-throat ethics. But it is not likely that we can effect any perceptible influence upon these only as the pebbles of truth, brought together by the patient and persistent search which we pursue and encourage, cemented by the pervasive law of life, will some time make more secure the foundation for the superstructure of society. Men of science are freely accused to-day of a *snobisme scientifique*. The reproach comes pretty hard and fast from certain highly deductive quarters peopled largely by conventional thinkers. And it is not always in French, either. We may make all allowances for criticisms of our work based on a point of view quite inconceivable to consecutive inductive reasoning, but the fact is quite evident that scientific snobs and scientific snobbishness do abound. A clergyman of my acquaintance asked a scientific friend about his work—what vast problems were now occupying his mind, and with utter and quite needless frankness the answer was—Repairing the conclusions I arrived at last year. That quickly served to point a sermon on the errancy and frailty of inductive science. But while the clergyman and the type of thought he stands for ought to know that progress in knowledge is never along direct lines, always in bewildering zigzags and pulsations which nevertheless end further forward than where they started, still the man of science does himself and his work an injustice in permitting himself to be exploited by agencies of popularization which are in essence entirely hostile to his mode of thought. So long as the whole pathway of science is bordered by the graves of cast-off theories and the atmosphere of to-day reeks with the aroma of new theories standing on probation and some in dire need of burial, it is wholesome to stop and reflect that these in their turn are likely to be inhumed with only rotting stones to mark the names of their progenitors.

We must recognize the fact that there is to-day a palpable reaction against the scientific mode for which we are ourselves to blame. The uplifting impulse given to every department of thought by the revival of the last decades of the nineteenth century is followed in this decade by a tremendous rebound. We may not all be fully conscious of this, as we are in some measure aloof from the pulse of other than scientific thought, but its proportions are well expressed by the formal and direct organization against the scientific mode embodied in such a masterly structure and so well-generaled an army as the eucharistic congress. Let us not deceive ourselves, this great body of serious-minded deductive thinkers marshaled for no other purpose than to counteract the logical and the obvious and to encourage reliance on the mysterious is not standing alone. The ease with which we make and unmake hypotheses, the finality with which we enunciate propositions, the autocratic statement of possibilities as facts unques-

tionably tend to lead pure science into disfavor wherever the humanities are more prevalent. This unhappily growing disposition to finality of statement, forgetting that we are still watching only the first scene of our drama, is, I think, essentially at fault, and we are ourselves largely to blame for an evidently increasing indifference with which the autocratic theories of science are now received. Nothing fails in our philosophy, there is neither want of majesty or fullness of truth in our objective. But men of science need to mend their manners.

The vast majority of all our communities to-day are almost wholly abandoned to commercial pursuits. However this truth may be paraphrased, the euphemism of "making a living" applies to the absorbing share of human activities, instigates the volume of our legislation, controls our international relations, impregnates conceptions of the conservation of natural resources and lies at the base of all the comforts and conveniences of life which we now enjoy in unparalleled measure. Commerce has been the advance agent of civilization, has reduced the wilderness to an Hesperidean garden, is the seat and source of power of one over another, whether of individual, state or nation. To the advance of commerce both science and art are under tribute, but to the commercial spirit itself the human world owes no good thing, no higher thinking, no advance of vital truth, certainly no ethical progress. It is, like the lust of blood, a primitive impulse which still overpowers the race and it gives the reflecting man some notion of how short is the way he has traveled toward excellence and how long the road that lies ahead. "Science," says a recent writer, "means service." That is applied science in its relation to the state, and it is well. But "science" has a profounder philosophy and blessed it is for humanity that the scientific *ideal* is sometimes wholly supreme and the *achievement* wholly incidental.

Experience and observation have deeply impressed me with the conviction that the most subtle intellect in our communities is that of the lawyer. I use the term only in its broadest and best sense. Trained to precise and consecutive thinking, averse to conclusions without full substantiation in fact or precedent, dealing with, at the bar, but perhaps, abhorring from the bench, hypotheses and reasonable presumptions, skilled in holding judgment in reserve and averse to dogmatic statement, the very intellectual practises which have effected these traits have helped to evolve a mental machine of extraordinarily delicate caliber and accurate register.

I can conceive of no finer training for a scholar of pure science than a course in the precedents and procedure of the law, if not carried too far, or if not that at least such intimate personal association with members of the legal profession as will help to irrigate the method of the scientific man with the mental prudence and reserve of the lawyer. Such a training would be better than Greek. The body of men that constitute the tribunals and the advocates in any country are the most influential element in conserving its culture. They are an anchor to windward. For human society they perform a function comparable only to that of the conservative mother in humanity; they ensure the perpetuation of the type.

But the lawyer is a dead wall to the progress of scientific truth. Deference to the common law, allegiance to the statute law, seem to have developed in him, both by training and association of ideas into the essentials of an instinct. We will not assume that the shortcomings of the con-

venances of the statutes are not more evident to him than to any other intellectual class in the community, or that he is blind to the mere expediencies of most legislation. Decent regard for the historic corpus of the law which embodies the experiences of self-governing people is essential to our community existence, but among the lawyers I have known best there seems to be ever a really inadequate apprehension of the essence of the law as expressed in Froude's definition of the term. "Our human laws," says Froude, "are but the copies, more or less imperfect, of the eternal laws so far as we can read them, and either succeed or promote our welfare, or tend to bring confusion and disaster, according as the legislator's insight has detected the true principle or has been distorted by ignorance or selfishness."

In fact, the natural law finds as yet feeble expression in our statutes. Certainly its recognized bearings are becoming incorporated into the laws of this land only as these recognized factors protect or guarantee the material rights of the people. But it is clear that neither the bare fact of legislation nor the decisions of the courts rest on any adequate conception of the workings of the natural law. Deference therefore to the written statute on the part of those whose intellectual energy is devoted to its interpretation or its execution, seems to have well-nigh suffocated the purpose to grasp the higher law on which governments built to endure must be established. Pure science which concerns itself in the search for the actual law regardless of its bearings on the present constitution of society, finds amongst this class in the community toleration, indeed, but little sympathy.

Of the three so-called learned professions in our early communities, the "doctors" constituted one. They do yet. To them we owe a debt which can not be expressed. Their contributions to a knowledge of the biologic law are immense, far greater indeed than many of that profession realize. Speaking generally and broadly, it is fair to say that the physicians and surgeons of our community have worked out to approaching completeness the anatomy and functions of the most intricate and highly specialized member of the animal creation. It is probably true that, with brilliant exceptions, they have not often looked long or seriously into organic nature beyond or below man, nor sought the solution of his complicated morphology in its earlier expressions or in its history. No doctor of medicine or of human anatomy would have conceived a theory of natural selection or any other procedure of organic evolution. It was bound to come either from the student of pure biology or paleontology or from the poet. Starting at the top instead of the bottom of the ladder of life, by empirical methods fraught with immense consequence to humanity, they have solved many of the mysteries of life.

Concretely viewed, the appealing merit of their part in the revelation of the natural law lies in its contribution to human happiness by the relief of human misery but abstractly this application is of no concern in the philosophy of nature. The physical man is an item in the scheme of life and he would be audacious indeed who would say he was more. But it is the most compelling fact of existence that we are that item and it is our comfort and progress that has been and are to be largely ensured by these conservators of our being. We may smile with some degree of indulgence over this monopolistic trade-mark of doctor. You who have earned your doctorates in science or laws by long years of close application to arduous work will find

your achievement unrecognized by the doctor of medicine who acquired his degree by a three or possibly four years course in some medical school only possibly of respectable grade. In the community, outside of the university circle, you will be addressed by them as Mister, preferably as Professor, and if the latter, with apparently no other distinction in mind than that you are of a peculiar social genus which for convenience and to avoid infringement of copyright is so designated and with broad enough application to embrace with you the dancing master, the friseur and the artist in fistics. It is perhaps reasonably true that in any community deprived of the academic atmosphere, that is in the average American community, there is among practitioners of medicine and surgery an indifference to or failure to grasp the purport of pure biologic research, so complete and dense as to be appalling, the more as it lies in a quarter where one might most reasonably hope for support and sympathetic concern. Compared with the intelligent manufacturer, the wholesale merchant, the banker, the stock broker, the modern farmer, the average physician or surgeon carries toward the ideals of our science and of geologic science as well, so far as the results involved are not strictly commercial, an air of indifference that partakes of the supercilious.

These are the chief elements in our human atmosphere with which the work of some of us must most often come into close contact. As for the two influential classes —the clergy and the teachers—taken together as one class, the educators, it must be said that the reacting influence upon pure science is least of all. Either they lead where the student of biologic science can not follow or they follow where the biologist leads.

At the opening meeting of this society a year ago an effort was made to present as an introductory to our work the broader bearings of paleontologic science in a form which might be read and understood of all men. It was thought, and wisely, I believe, that in this way the society might declare at the outset a platform of helpfulness to the advance of human knowledge and thus to the progress of culture. Those of us who were privileged to listen to this concourse of authoritative expression on the achievements and potentialities of the science must have felt that such a summation of its purport was needed. I believe this society, now in its inception, would do well to keep before its eyes as the real objective of its existence, the fact that it must make itself immediately contributory to human culture and popular instruction if it is to achieve a worthy existence. In our varied special interests we are somewhat forced to overlook these broader and perhaps more humane applications in the conviction that our work is its own justification, no matter how constrained its boundaries. It is even so, but it was also a singular commentary I fear on the mental attitude of some of us toward what is really the supreme objective of our work that the council of this society decided not to publish this series of illuminating papers on the aspects of paleontology—and all the more a matter for congratulation that my successor to this office assured their publication in a magazine of wider and more popular circulation than the bulletin of the society.[2]

Rarely has there been given in any one place as satisfactory a summary of the philosophy and achievement of this sci-

[2] The papers delivered at the Conference on the Aspects of Paleontology at the first (Cambridge) meeting of the society have appeared in various numbers of *The Popular Science Monthly* for 1910.

ence. Competent men in all cases and in many equipped to speak with supreme authority, presented the propositions of the science from the point of view of their special lines of interest, and though on every topic brought under consideration much more might have been effectively said, yet the sum of the matter was to set before the mind, in part at least in inimitable form, the scope of the science. The very nature and diversity of our interests are both our weakness and our strength. Our nature is trinitarian: whenever we cease to be held together by the centripety of a common broad unity, to keep our feet together on the platform of common concern, then we fly apart into our lesser orbits. If I could venture therefore out of my experience with the larger world, to urge one consideration of paramount concern on my successors in this office, it would be that on these occasions of annual reunion sight never be lost of our major purpose and no risk ever be invited of swamping our unity in the sea of details. Let us not blind ourselves to the beauty of the forest by seeing only the trees in it.

II. THE SIGNIFICANCE OF CERTAIN EARLY PARASITIC CONDITIONS

I propose now to turn your attention with reasonable brevity, and as a conclusion of this address, to a series of considerations which I am disposed to believe, when more fully illuminated by a patient and persistent accumulation of facts, may have a wider application to interests of immediate moment to society. In the acquisition of the evidences of the earliest phases of the parasitic or dependent condition of life I have been somewhat assiduous —enough at least to realize, as every one must in entering such a field, how much remains to be acquired and how many illustrations of it still lie in our great museums unrecognized or unstudied. The appreciation of the parasitic conditions of to-day depends so wholly on the adaptations in the physiology and soft anatomy of organisms, that to seek such clues as these among the rocks may seem like entering a blind cavern without a torch. Volumes have been written and volumes more would be necessary to portray the aspects of dependent life in living nature from the simplest organic subjection among the bacteria to the most complicated expression in human society, and with these before our eyes I believe it possible eventually to resolve from the record of the past the problems of the origination of such dependent conditions.

Whatever attitude you and I may take as paleontologists toward the conceptions of Le Dantec and his followers, that the whole panorama of life is being renewed from its beginning every day in the scenes of the present world, these at least can not cover the transactions of organic dependence in which the time element is the most illuminating factor in its existence. What I have to offer at present is a very brief statement of a few suggestive facts and of certain justifiable inferences of broader scope which rank, I believe, in the category of continuous effects.

In the effort to find the real significance of this vital relation I have purposely confined my observations, so far as the fossils are concerned, to those within the Paleozoic. There have entered into the literature of fossil dependents straggling and usually quite incidental records of symbiosis in the eras later than the Paleozoic, but none of these eras which approach near to the present has added, so far as my knowledge extends, any essential clues to the origination of this condition. This is naturally so, for the advanced life of the Neozoic

times too closely approximates that of the present without carrying with it the solutions suggested by the soft bodies of to-day. Our statement of inferences in this problem must be very guarded because of our being bound down only to such records as the rocks have been able to retain. But lest these evidences be underestimated, let me insist that nature has dealt kindly in the retention of the most intimate structures and here again, so far as skeletal remains go, we can safely fall back on her beneficence.

Purely symbiotic mutualism is of very ancient date and we find frequent evidence of commensalism far back in Paleozoic faunas, of the same type of combinations as abound in the seas of to-day. Thus there are few commoner examples of mutual associations in the present sea than those of the worms and corals, the worms and sponges, sessile cirripeds and corals, and I have elsewhere indicated a number of such occurrences in the Paleozoic. There are the diffuse slightly curved tubes of the worm *Gitonia sipho* which traverse the fine-celled stratiform colonies of the coral Stromatopora in the Silurian and Devonian, the spiral worm Streptindytes of graceful form in the Silurian Stromatopora and one of heavier habit described by Calvin in the large-celled coral Acervularia of the middle Devonian. The open apertures of tubes of *Gitonia coral-lophila* are often to be seen projecting from the cups and even the sides of the coral *Zaphrentis* and its allies. These are simple associations in which the partners are and have always been equally and mutually dependent, both having begun life as independent free swimming beings. Among these commensal worms and corals there is one association that invites particular notice in passing, for so far as I know the present fauna, its parallel is not recorded. This is the compound coral *Pleurodictyum* and its worm *Hicetes*—an association long known to students of the Devonian, but not till recently, with the help of carefully prepared materials fully comprehended. *Pleurodictyum* is a compound favosite growing in small, large-celled lens-shaped coralla of the size of a brussels sprout. Its commonest species are *Pleurodictyum problematicum* of the European Coblentzian and *Pleurodictyum stylo-porum* of the Hamilton shales of America. The sessile thecated base of the corallum in both these species seems to have attached itself to the rock or mud of the sea bottom in a way that involved the formation of no special cicatrix or scar of attachment. My observations lead me to the conclusion that in the Coblentzian species such form of attachment occurred in about one half of the individuals and in perhaps somewhat less than one half in the Hamilton species. The rest attached themselves at the beginning of the sessile stage to some organic object, some dead shell on the sea bottom. In the Hamilton species in approximately one fifth of the cases, this dead shell was usually a gastropod of the genus *Pleurotomaria* or *Cyclonema;* in the remaining four fifths, the shell was always the same shell, a species of the gastropod *Loxonema* (*delphicola = hamiltonensis*). I have not recorded an instance of the coral being attached to any other than a gastropod, shell. These are rather broad statements but are based on the examination of several hundred examples and are an approximate expression. These bases of attachment were of course dead shells which the free-swimming coral larvæ *selected.* I have italicized the word "selected" as, however else one may be disposed to interpret the phenomenon, the act bears a strange resemblance to conscious choice. The Coblentzian *Pleurodictyum* so similar in struc-

ture as almost to veil specific difference, has however chosen differently. I can not say that I have seen a wholly convincing number of German specimens, though probably I have had opportunity, by the help of collectors and the collections of many museums, of seeing as many as any one else, and I have further had the benefit of accounts given by various workers. An approximate half of the German specimens, rather more than less, are unattached. For the remainder I have seen none that is not attached to the brachiopod *Chonetes sarcinulatus.*[3] However these very strange facts may be expressed in accurate percentages, the palpable evidence remains, that the free and independent larvæ of these closely allied species of the same genus of coral have in one part of the world unerringly taken a brachiopod shell for a *pou sto,* while in another perhaps actually coeval sea they have not chosen a brachiopod but always a gastropod, and usually one and the same species of gastropod, for their sedentary maturity.

Neither of these species of coral embryos was deprived of a choice. The sea bottom of the Coblentzian and of the Hamilton stages teemed with all varieties of invertebrate remains. Gastropods and brachiopods flourished alike on both. An act of choice could not pertain to the degenerate and fixed adult condition of growth; but it is a matter of supreme interest that such an apparent act, in whatever psychological category it may find its place, did manifest itself in the higher stage of normal inheritance, the free and independent condition of early life.

I presume that in all the phenomena of commensalism which the early faunas of the earth have exhibited, the concurrence of the worm and the coral is the commonest. Yet I have not observed it in faunas earlier than the Silurian, with possible reservation for the Ordovician. The Cambrian faunas contain the vital elements necessary for such cohabitation, but have not shown any indication that this habit of interdependence had been formed so early, even though the degenerate condition of attachment had been abundantly acquired then and doubtless long before[3] I shall not enter into needless detail regarding other mutual associations of these early days, citing briefly, for convenience of reference to those who are interested, the cases of the corals *Favosites* and *Amplexus* recorded from the Niagaran of Iowa, of a worm in the bodies of the glass sponges, *Hydnoceras* and *Prismodictya* of the upper Devonian, of the barnacle *Paleocreusia* in a colony of *Favosites,* from the lower middle Devonian Onondaga limestone.

In late stages of the Ordovician there are certain crinoids of the genus *Glyptocrinus* and other long-armed inadunates which when found with arms drawn together hold within them a round-mouthed snail shell of the genus *Cyclonema.* I have examined a number of these associations, but have seen in none of this age any evidence of actual attachment of the shell to the proct or any other part of the crinoid. Evidently here the association was a loose one, the snail feeding on the fecal waste of the crinoid, hanging around the back door like a dog at a garbage pail. It was not long after the inception of this habit, however, that an actual fixation ensued, and there are well-known cases in the faunas of the Silurian of both crinoids and cystids cemented by a muscular attachment over the proct of the host, so firmly as to hold

[3] *Pleurodictyum* of like Devonic age and with its *Hicetes* occurs both on the Bosphorus and the Maecurú River, Brazil, but we know nothing yet of its habit of fixation.

[3] At this meeting of the society Dr. C. D. Walcott exhibited specimens of a Cambrian worm commensal with medusa.

its place during all the procedure of fossilization. We can not say how enduring this condition of deject dependence was in the life of the individual of the Silurian, but the Devonian faunas and more especially those of the early Carboniferous when the crinoidea attained their maximum profusion, at once furnish abundant and well-known facts indicating that this habit had at least become so fixed that it began with a very early juvenile stage and continued probably till released by death. The domes of many crinoids in these faunas have shown by successive elliptical scars, of which the proct is the focus, left by growing shells, that the snail remained permanently and solidly devoted to this habit for probably all the later stages of its life. This is distinctly a case of true parasitism; not merely of that degeneration resulting from dependence which is universal in nature, but of absolute and abject dependence on the vital functions of another. During the maximum of the crinoids as well as of these capulids or limpets in the early Carboniferous time the attachment seems to have been not occasional, but to have attained the character of an actual habitude to which all members of the Capulidæ were liable even though not all really practised it. This singular condition endured during millions of years from its inception in the Silurian to its climax, but from the time of its general prevalence in the early Carboniferous on through the closing stages of the Paleozoic and through the long following ages up to the present we seem to lack any shred of evidence of its continuation. In very fact it seems as if in the long panorama of life through the varying phases of this great time interval, for the first time in all our knowledge of the parasitic life in all circles of existence (except where this has been influenced by the higher motives which govern humanity), that here nature had demonstrated the power in herself to rebound from such tendencies to degeneration, to give her creatures a sharp turn about and start them again on the upward path. What a splendid significance of salvation it would be for the tremendous majority of nature's apparently doomed races if there should lie herein even a slender ray of hope for the recovery of tendencies toward the normal upward progress which so many organisms have lost by the supervention of degenerate habits. We may not be entirely sure that this ray of hope for a lost nature really exists, for even though we have no evidence of the parasitic dependence of the gastropods upon the Echinoderma in all the ages of the Mesozoic and the Cenozoic, we do know that in the present sea all parasitic gastropods are parasites on the Echinoderma, the class to which the crinoids belong.

Crinoids are few to-day; to my knowledge no one has recorded on them the presence of parasitic snails, but their close relatives, the star-fish and sea urchins, are still beset by gastropods, often so modified by their degeneracy that their nature is hardly recognizable, and this parasitism too is fixed and beyond repair. In these cases which have been particularly studied by the Sarasin brothers, the parasite is still of the limpet type of structure, whether the host be starfish or sea urchin. There is a palpable vast difference in degree between this particular form of parasitism to-day and the association of limpet and crinoid in the days of the Carboniferous seas, the later irremediable, the earlier still not beyond repair, well established but clearly not beyond the control of the dependent. The great gap of positive evidence in the profoundly studied Mesozoic and Tertiary faunas which certainly should have af-

forded such factors had they ever existed, leaves us a reasonable propriety to construe the cessation of this parasitism as actually accomplished in Carboniferous times, and the recurrence of like associations among allied animals of the present an essentially new adaptation of the two. For nature has made these adaptations easily and degeneracy to the parasitic condition is one of the facile procedures of life, moving on apace with ever-increasing momentum toward race destruction. Much more natural, much more logical, much better justified by well-known procedure in these easy adaptations, is such a proposition, than that after millions of ages of suppression the ancient abject dependence of the limpet on the crinoid should have reappeared as a direct inheritance out of the remote past. It is to this point I would direct the major purport of this paper, for aside from all other questions of ancient parasitic conditions there is none yet of so profound biologic significance as this question raised by the parasitism of the Paleozoic gastropods on the crinoids. Did the snails get over it? Did they conquer this habit of dependence and return to normal independence? Granted that the parasitic condition is but an expression of adaptation; yet it unfailingly involves degeneration. Did the gastropod rebound from its degenerate adaptation and return toward the normal living which has helped to perpetuate its stock abundantly to the present in more self-reliant expression? I believe that all the positive and all the negative evidence we can now adduce on this deeply important subject favors the presumption that the habit was abandoned or at least, to speak in terms of simple casuistry, was lost. We may give the counter proposition all the weight to which it is entitled, and I believe we ask too much of both evidence and presumption to seek in it a recrudescence of a transmitted acquired habit in the present after the lapse of untold millions of years between its appearances.

Living nature can of its very essence present us with no such proposition as this. Here the time element is of paramount importance and we are confronted then here by the single example in all known nature below man of the possible recovery of a normal upright tendency in organic living after a period of degeneration, of a case of what appears to be, deducting due allowance for possible uncovered records among profoundly studied geological faunas, an actual rebound from an adaptation to evil living back to the normal uprightness of independent life by the intervention of natural forces only. I might cite to you additional interesting cases of the parasitic habit in Paleozoic organisms, but none which can now amplify the special point I have had in mind in bringing these succinctly to your notice. For doubtless if nature could and did turn even an invertebrate sinner from the error of its ways once, she can do it again; she doubtless has done it more than once and her influences directed toward the demolition of such degenerating adaptations could not be restricted solely to lowly forms of life. The supreme merit of the striking illustration I have been able to bring to your attention lies in the panorama of its history, its inception, its progress, climax and probable extinction. Even though it may now stand alone, the argument from it could not be much altered nor greatly strengthened by supplementary cases, however it might be clarified by the verification of the negative evidence in the ages between the Paleozoic and the present. But so far is the evidence good and the logic sound that I need not hesitate to intimate that the parallelism between organic

and social parasitism is not a similitude. Organic degeneracy and degeneracy in human society are not, I take it, a mere equivalence in terms, but a result in effects through stages of adaptation entirely comprehensible. Sin, then, the expression of degeneracy, we may reasonably hope to believe and perhaps eventually to demonstrate from the facts of our science, is an error whose cure may lie within our own inherent impulses and whose existence may be terminated with the stronger growth of our intellectual and moral perceptions entirely within the sphere of nature herself.

John M. Clarke

THE DISTRIBUTION OF THE COLLEGE MEN

The *Yale News* has made an analysis of "Who's Who in America" for 1910–11 and finds in the book the names of 8,529 college men. The data for fourteen institutions are given as follows:

	Law	Medicine	Education	Science	Engineering	Ministry	Writing	Mining Eng.	Journalism	Finance and Business	Government Service	Retired	Total
Harvard..	158	92	175	111	25	48	77	6	59	41	43	15	813
Columbia.	62	29	53	32	13	19	14	7	10	15	7	—	261
Yale.....	130	51	131	71	24	84	49	4	43	41	42	10	681
Michigan.	57	28	69	47	12	9	17	2	8	8	24	10	271
Virginia..	38	19	27	11	2	8	2	—	1	2	11	1	122
Wesleyan	14	3	37	12	2	32	4	—	6	1	7	3	121
Princeton	50	20	41	11	3	46	14	1	17	8	8	1	210
R. P. I....	2	—	—	1	14	—	—	—	—	—	—	—	17
Amherst..	23	10	65	29	5	36	12	—	10	9	4	2	205
Williams.	21	5	27	10	1	30	8	—	9	4	7	1	123
Chicago...	10	5	32	18	—	7	4	—	4	5	2	1	88
M. I. T...	—	3	4	16	20	1	3	—	2	2	2	—	52
Cornell ...	15	9	38	39	27	7	11	—	10	3	5	3	167
Penn	32	53	29	18	11	15	10	3	11	4	9	3	200

SCIENTIFIC NOTES AND NEWS

The officers of the American Museum of Natural History, who were elected at the annual meeting of the board, held February 13, 1911, are as follows: *President,* Henry Fairfield Osborn; *First Vice-president,* Cleveland H. Dodge; *Second Vice-president,* J. Pierpont Morgan, Jr.; *Treasurer,* Charles Lanier; *Secretary,* Archer M. Huntington; *Acting Director,* Charles H. Townsend; *Assistant Treasurer,* United States Trust Company of New York; *Assistant Secretary,* George H. Sherwood. By unanimous vote of the trustees, Professor Bashford Dean, Columbia University, was reinstated in his post as curator of ichthyology and herpetology. The trustees also promoted Dr. W. D. Matthew from acting curator to curator of the department of vertebrate paleontology. Barnum Brown, assistant curator of fossil reptiles and Walter Granger, assistant curator of fossil mammals, become associate curators.

For the meeting of the British Association for the Advancement of Science, which is to take place this year at Portsmouth on August 30 and following days, under the presidency of Professor Sir William Ramsay, K.C.B., F.R.S., the following presidents have been appointed to the various sections: *Mathematical and Physical Science,* Professor H. H. Turner, D.Sc., F.R.S.; *Chemistry,* Professor J. Walker, D.Sc., F.R.S.; *Geology,* A. Harker, M.A., F.R.S.; *Zoology,* Professor D'Arcy W. Thompson, C.B.; *Geography,* Col. C. F. Close, R.E., C.M.G.; *Economic Science and Statistics,* Hon. W. Pember Reeves; *Engineering,* Professor J. H. Biles, LL.D.; *Anthropology,* Dr. W. H. R. Rivers, F.R.S.; *Physiology,* Professor J. S. Macdonald; *Botany,* Professor F. E. Weiss, D.Sc., with W. Bateson, F.R.S., as *chairman of the Sub-section of Agriculture; Educational Science,* Rt. Rev. J. E. C. Welldon, D.D.

A portrait of Sir William Crookes by Mr. E. A. Walton, has been presented to the Royal Society.

Professor D. Oliver, F.R.S., formerly keeper of the herbarium and library of the Kew Gardens, known for his important contributions to botany, celebrated his eightieth birthday on February 5.

Membership in the Prussian House of Lords has been conferred on Dr. Wilhelm Waldeyer, professor of anatomy in the University of Berlin.

The gold medal of the Royal Astronomical Society has been presented to Dr. P. H.

Cowell, for his contributions to the lunar theory and gravitational astronomy.

THE Hopkins prize has been awarded by the Cambridge Philosophical Society to Professor J. H. Poynting, F.R.S., of the University of Birmingham, for his researches on the transmission of energy in the electric field and on the pressure exerted by radiation.

THE *Journal* of the American Medical Association notes that in addition to the Nicholas Senn high school, the site for which has been secured, Chicago schools are named after the following physicians: Daniel Brainard, William H. Byford, Nathan Smith Davis and Charles Warrington Earle.

MR. F. W. DYSON, F.R.S., astronomer royal, and Surgeon-General Sir Alfred Keogh, K.C.B., rector of the Imperial College of Science and Technology, have been elected members of the Athenæum Club for their scientific work.

M. G. BIGOURDAN has been appointed president of the Bureau des longitudes, Paris. M. B. Baillaud is the vice-president and M. H. Andoyer, the secretary.

DR. HENRY SKINNER has resigned the editorship of *Entomological News,* taking effect with the number for December, 1910. He had been editor continuously since March, 1890, since which time the journal increased in size from 168 pages and no plates to 484 pages and 14 plates per annum. The journal is in charge of the joint publication committee of the Entomological Section of the Academy of Natural Sciences of Philadelphia and of the American Entomological Society. This committee accepted Dr. Skinner's resignation with regret and elected Dr. Philip P. Calvert (associate editor since January, 1893) editor, Ezra T. Cresson, Jr. associate editor, and Dr. Skinner editor emeritus.

AT a meeting of the American Philosophical Society on March 3, Professor F. M. Jaeger, of the University of Groningen, will read a paper "On Fluid Crystals and Bi-refringent Liquids."

PROFESSOR L. H. BAILEY, dean of the Agricultural College of the State of New York, appeared as the ninth lecturer upon the J. C. Campbell Foundation of the Sigma Xi Society of the Ohio State University on the evening of Wednesday, February 8. He spoke upon the subject "The Country Life Movement."

DR. M. P. RAVENEL, head of the department of bacteriology, represented the University of Wisconsin at the meeting of the American Association of School Hygiene, where he read a paper on "The Function of University Faculty Committees on Hygiene."

DR. M. B. THOMAS, of Wabash College, recently lectured to the Illinois State Horticultural Society on "The Nature of Parasitic Fungi and their Relation to Their Hosts."

THIRTY-SEVEN professors from the University of Wisconsin appear on the program of the annual meeting of the Wisconsin Academy of Sciences, Arts and Letters, the Wisconsin Archeological Society, the Wisconsin Mycological Society and the Wisconsin Natural History Society, which opened in Madison, February 17. The opening address of the first day was on "The Relation between Area and Temperature of Lakes," by Dean Edward A. Birge, of the college of letters and science. Fifteen addresses were made by members of the university departments of geology, botany and zoology.

DR. EDWARD HITCHCOCK, for fifty years professor of hygiene and physical education at Amherst College, died on February 15, aged eighty-two years.

PROFESSOR FRANK J. PHILLIPS, head of the department of forestry in the University of Nebraska since 1907, died on February 13, at his home in Lincoln, Nebraska. He was a graduate of the Michigan Agricultural College and the University of Michigan, and had spent several years in the United States Forest Service before his connection with the University of Nebraska. He was the author of a number of important scientific papers and was one of the most brilliant teachers that the university has known.

DR. M. WILHELM MEYER, known for his contributions to astronomy, and his efforts to

popularize this science, has died at Meran at the age of fifty-eight years.

The death is also announced of Dr. Sigmund Grundfienger, formerly professor of mathematics at Darmstadt.

Herr Leopold Koppel, a Berlin banker, has given $175,000 for the erection of a research institute for physical chemistry in Berlin and will make a further gift of $87,500 extended over the next ten years for maintenance.

The senate has passed the Weeks bill providing for the establishment of a forest reserve in the Appalachian Mountains by a vote of 57 to 9. The bill is applicable from Maine to the Gulf of Mexico, on the eastern seaboard. It was passed by the house at the last session. The bill gives the consent of Congress to the states to enter into an agreement among themselves for the purpose of conserving the forests and water supply of each, and appropriates $200,000 to enable the secretary of agriculture to cooperate with such states in giving fire protection. In addition, the bill appropriates $1,000,000 for one year, and not more than $2,000,000 for each year thereafter, until 1915, for surveys, examinations and acquirement of lands located at the headwaters of streams which are being or may be developed for navigable purposes. A commission composed of the secretaries of war, interior and agriculture and of two members each of the house and senate is created to acquire these lands subsequent to an examination by the Geological Survey.

It is reported in *Nature* that at a special general meeting of the Geological Society of London on January 25, the following resolutions were passed: (1) That the space now occupied by the museum be made available for the extension of the library. (2) That it is desirable that the society's collections of fossils, minerals and rocks, with certain exceptions to be subsequently specified, be offered to one or more of the national museums, provided that guarantees be obtained that the specimens will be properly registered and rendered available for scientific purposes. (3) That it is not desirable that the society should accept money for any part of the collections, or in consideration of them. (4) That the council be empowered to approach such institution, or institutions, with the view of carrying the above resolutions into effect, and that the council shall call another special general meeting to express approval or otherwise of the arrangement proposed.

The late Dr. Huchard has bequeathed to the Académie de médecine a sum of $20,000 to found an annual prize to aid young students, who like his son, Marcel Huchard, in whose memory the prize is founded, are victims to professional devotion and to enable them to continue their studies.

A catechism on bovine tuberculosis is the title of a circular of information issued by Dean H. L. Russell, of the college of agriculture of the University of Wisconsin, and Professor E. G. Hastings, of the university experiment station. This bulletin presents over sixty questions gleaned from inquiries received at the college with brief answers based on the best scientific knowledge of bovine tuberculosis. Complete instructions for determining the presence of the disease through the administering of the tuberculin test are given, together with hints on how to control the disease in a large herd.

The operation of amblygonite mines in South Dakota during the last two years has reduced the price of lithium carbonate from $2.50 to 50 cents a pound, and large quantities have been sold at still lower prices. The carbonate is the lithium product most used and is employed in making storage batteries and fireworks and for medicinal purposes. Lithium, the metal, has no practical use. Lithium bromide is used to some extent in photography and in medicine. In an advance chapter of the United States Geological Survey's volume entitled "Mineral Resources of the United States, Calendar Year 1909," prepared by Frank L. Hess, the occurrence and extraction of lithium ores are discussed. In 1909 several carloads of amblygonite were taken from mines at Keystone, S. D., and a few carloads of spodumene were shipped from the same locality by another company. Practically all

the lithium compounds prepared in the United States in 1909 were extracted from these ores.

THE department of public health at the American Museum of Natural History has equipped a laboratory to serve as a central bureau for the preservation and distribution of bacterial cultures of both pathogenic and non-pathogenic organisms, and particularly of types of new forms and varieties. It is hoped that the laboratories of medical schools, colleges, boards of health, agricultural experiment stations, etc., and those engaged in biochemical work of all sorts, will furnish the museum with cultures at present in their possession, and the laboratory is now ready to receive and care for such cultures. Only organisms which have been identified and which have a definite history are desired as a rule; but in the case of rare species, like the organisms of certain tropical diseases, this rule may be departed from. The laboratory can not undertake to maintain more than fifteen different strains of any particular form. Types of new species and varieties are particularly desired at the present time and as they may be isolated in the future. The laboratory plans also to keep on file descriptions of bacterial species in print or arranged in the form of the standard card and will be grateful for copies of any such descriptions. Descriptions filed in the department will be carefully preserved and living cultures will be kept in good condition, so far as possible, and will be supplied at all times without charge to corresponding laboratories and furnished so far as possible and with a reasonable charge to schools and other institutions which may desire cultures. The laboratory, of course, can not undertake to keep on the difficultly-cultivable bacteria, such as can be maintained only for a few weeks after isolation from the body; neither can it at present supply virulent cultures which rapidly lose their virulence under laboratory conditions. It should, however, be able to furnish cultures of organisms of all the ordinary types, which can be maintained under cultivation. Pathogenic forms will be sent only to properly qualified persons.

UNIVERSITY AND EDUCATIONAL NEWS

IT is announced that Professor Hans Meyer has presented 150,000 Marks to the University of Leipzig for the laboratory of experimental psychology established by Professor Wilhelm Wundt.

THE New Hampshire College of Agriculture and Mechanics Arts will continue under that name, the house of representatives of the state not having approved a change to the University of New Hampshire.

A SCHOOL for forest rangers to consist of a two-year course, the winters of which will be spent at the university, and the summers in practical work on the state forest reserves, or in lumbering operations in the field, is proposed to be started at the University of Wisconsin. Outside the Pennsylvania ranger school and those established by the federal forest service in connection with some of the far western institutions, no attempt has been made to meet the demand for expert foresters. It is thought that much of the work of the course in forestry could be given in the present departments of the colleges of engineering and agriculture. The state department of forestry would probably need all the trained men that the school of forestry could turn out for a number of years.

THE newspapers report that following the student disorders in Russia one hundred and twelve professors have resigned or been dismissed. It is further said that the ministry of education in order to attract professors in other countries plans to establish temporary Russian schools of law in Berlin and Paris, a school of natural sciences at Heidelberg and one of medicine at Paris.

IT is announced that Dr. George E. MacLean has resigned the presidency of the State University of Iowa, and that Dr. Edmund A. Engler has resigned the presidency of the Worcester Polytechnic Institute.

MR. FRANK HOWSON, of the University of Durham, has been appointed lecturer in physiology at Sydney, New South Wales.

DISCUSSION AND CORRESPONDENCE

BOTANICAL EVIDENCE OF COASTAL SUBSIDENCE

In a recent number of Science[1] Mr. H. H. Bartlett, writing under the above title, criticizes an earlier paper of my own concerning "The Supposed Recent Subsidence of the Massachusetts and New Jersey Coasts,"[2] on the ground that it represents hasty conclusions based on the examination of one locality where conditions are far from typical. Mr. Bartlett is of the opinion that the hypothesis there advanced to account for the appearances of recent subsidence along the coasts in question has "as a matter of fact . . . no bearing whatever on most of the evidence which has been offered."

Inasmuch as the brevity of my paper may be responsible for Mr. Bartlett's failure fully to understand it, perhaps a few words as to the development of the hypothesis presented in that paper may serve to make the hypothesis itself clearer. The writer's active interest in the problem of subsidence began a number of years ago when he was retained by counsel for the commonwealth of Massachusetts, in a case involving title to lands now below high tide level, to determine if possible the nature and extent of the recent subsidence along the Massachusetts coast. He entered upon this investigation with the belief that recent coastal subsidence in this district was a fact, well established by the studies of various students of shoreline phenomena; and his efforts were mainly directed toward ascertaining whether the subsidence was continuous in time and regular in rate, or whether it was intermittent and at varying rates. In connection with this investigation examination was made of numerous publications on recent changes of level in the United States and Canada, and many foreign reports on the same topic; and so fair familiarity with the literature of the subject was gained.

Even before undertaking the above investigation, certain geologically recent changes in the form of the shorelines about Boston had attracted the writer's attention; and in the succeeding years more or less consideration has been given to this subject. Several of his students have made special studies of selected areas along the coast, in each case giving attention to the problem of coastal subsidence. Their observations and his own, supplemented by studies of maps of certain areas not visited, led the writer to conclude that the coast had remained essentially stable for a long period of years; and that if any considerable subsidence had taken place, it must have occurred long ago.

The botanical evidence of recent subsidence was still a puzzle. Manifestly, the uppermost layers of the *Spartina* turf, and the stumps of cedars and other trees exposed in the marshes, could not be due to a remote subsidence, even if the lower layers of turf and the more deeply buried stumps had been depressed long ago. Careful attention has been given to this phase of the problem, and in this connection let me express my great indebtedness to Dr. Charles A. Davis, who gave me the opportunity to accompany him on several of his field excursions, and who took the time to visit with me two or three localities where I had studied the physiographic features of the shoreline. On these excursions I became fairly familiar with the botanical evidence of subsidence, and with the interesting methods of investigation which Dr. Davis has developed for the study of salt marsh deposits. By means of the ingenious peat sampler devised by Dr. Davis, the writer has endeavored to increase his knowledge of the structure of several of our marshes; and one of his students has made a detailed series of sections across the marshes at the mouth of the Neponset River, which will be referred to in a future publication.

As a result of these studies, it seemed to the writer that while the lower portions of the marsh deposits might indicate subsidence in times long past, the upper portions (the portions which furnish supposed evidence of *recent* subsidence) might be explained in either one of two ways: they might represent a resumption of the downward movement of the coastal region in recent times, after a long

[1] Science, N. S., XXXIII., 1911, 29–31.

[2] Science, N. S., XXXII., 1910, 721–723.

halt; or they might be explained as the result of local fluctuations of marsh level or of tide level, independent of a downward movement of the coastal region. The long halt in the supposed downward movement seemed to be demanded by the physiographic evidence.

Observations in the Scituate-Marshfield region revealed a cause of fluctuations in high tide level which appeared competent to explain most, if not all, of the evidences of recent subsidence. After a consideration of the tidal conditions along the Atlantic coast, and a study of the nature of the high tide surface about the coasts of England, as reported by Wheeler and others, the conclusion was reached that the conditions in the Scituate region had been repeated in a greater or less degree all along our coasts in recent geological times, as a necessary consequence of the nature of the high tide surface and the changes effected by wave action on all shores, particularly those composed of poorly consolidated materials. The cause seemed competent to explain a deposit of peat varying in thickness from a few inches to a possible maximum of 15 feet or more, according to the former range of the tides. As all of the supposed evidence of *recent* subsidence on the Massachusetts and New Jersey coasts are, so far as known to the writer, capable of explanation on the basis of a fluctuating high tide surface, and as the conditions on those coasts make such fluctuations in the past a seeming necessity; and as the physiographic evidence, on the Massachusetts coast at least, points to a long period of coastal stability in recent times, the conclusion seems reasonable that, while subsidence in the past may have occurred, the evidence of recent subsidence in these two areas is not decisive. Further study convinces me of the correctness of that conclusion. But whether it is correct or not, it was not reached with undue haste, nor was the botanical evidence of subsidence "lightly disregarded."

When a problem that has been discussed for many years, on the supposition that it involves but x factors, is found really to involve $x+1$ factors, all of the earlier conclusions should be carefully reconsidered; not that they are necessarily wrong, but because it can not be known that they are right until the additional factor is fully considered along with the others. The problem of recent subsidence of the Atlantic coast has long been discussed; but the importance of a high tide surface which fluctuates with changes in the form of the shoreline, as a possible explanation of this apparent subsidence, does not appear to have been considered in the published discussions of the problem. It is an element of possible value in all cases of tidal shores which are irregular in outline or which are bordered by barrier beaches. Hence all conclusions which have been reached in regard to recent subsidence in such cases ought to be revised in order to take this element into account. What the result of revision in each case will be can not be foretold; and for this very reason the revision seems the more necessary. The writer is attempting such a revision for several localities, without prejudice in favor of any particular conclusion, and in most of the cases without any idea as to what the final result will be.

My statement as to the inconclusiveness of the evidence of recent subsidence on the Massachusetts and New Jersey coasts, was made after a careful reading of Mr. Bartlett's very interesting paper on the sub-marine *Chamæcyparis* bog at Woods Hole, Mass. Although this paper presented a most ingenious and interesting argument in favor of recent subsidence, the validity of the argument depended upon certain assumptions which seem to me untenable. It was not the object of my brief paper to discuss the voluminous evidence in favor of recent subsidence, but rather to make a short preliminary announcement of an hypothesis which appeared to be of considerable importance; for this reason I did not deal specifically with Mr. Bartlett's observations. Full attention will be given to all accessible evidence in favor of recent subsidence in a future publication.

A careful reading of Mr. Bartlett's criticism of my paper leads me to think that he has failed to discriminate sufficiently between low-lying peat deposits which may be of consid-

erable antiquity, and the uppermost deposits unquestionably of modern date which alone can be invoked as evidence of recent subsidence. It would appear, also, that he has not clearly understood my paper; for it is otherwise difficult to account for such statements as "the hypothesis of a fluctuating high-tide level has no possible application" to the botanical evidence of present subsidence as presented by Dr. Davis; or that the hypothesis "has no bearing whatever on most of the evidence which has been offered." Surely if the height of ordinary high tides gradually rises one or more feet as the result of changes in the form of the shoreline without change in the level of the land, this increase in tidal height will produce all of the phenomena which would be produced by actual subsidence of the land, "now going on." The task which confronts the student of shoreline changes involves a discrimination between different causes producing like results; and the best method of making such a discrimination is, in the opinion of the writer, to deduce the character and magnitude of the results which each hypothetical cause is theoretically capable of producing under the varying conditions which exist along an irregular coast, and then to compare the deductions with the actual phenomena as observed in the field. So far as I have carried such an analysis for the Massachusetts coast, the evidences of supposed recent subsidence are all capable of interpretation on the basis of a fluctuating high tide surface; whereas physiographic features of much importance seem incompatible with the theory of continued recent subsidence.

D. W. Johnson

Miastor and Embryology

Reproduction by insect larvæ, a form of parthenogenesis known as pedogenesis, is extremely interesting. The writer was fortunate last fall in finding, in the partially decayed chestnut bark of a rail fence, numerous *Miastor* larvæ, forms not previously recognized in this country, though several species and representatives of allied genera have been studied by a few Europeans. *Miastor* larvæ and their allies should be of great service to teachers of zoology and biology, since they admit of the study at first hand of this interesting phenomenon. It is possible, with a no more elaborate outfit than an ordinary student's microscope equipped with a three-fourths objective, a microscopic slide and a few cover-glasses, to observe the vital activities of the young larva, to see the muscular, respiratory, digestive and nervous systems, to identify the ovaries and to watch the gradual development of the semi-transparent embryos within the body of the living mother larva. Furthermore, these forms are well adapted to more exact histological methods, being soft and therefore excellent subjects for serial sections and stains, particularly as it is comparatively easy to secure from one colony a series of individuals representing different stages of development.

Aside from the interest attached to their morphology and biology, there are other considerations which should appeal strongly to the teacher of zoology. These larvæ are widely distributed and, with an understanding of their habits, there should be little difficulty in finding them. They are small, and a piece of wood six inches long, three inches wide and half an inch thick, may contain or produce enough material for a fair-sized section or class. The larvæ are prolific and, under favorable conditions, would probably multiply at any season of the year. This is certainly true of the fall, the early winter and the spring. They can be kept alive for at least a month in microscopic cells, and with care a larval generation will develop in such restricted quarters. We have kept larvæ healthy and multiplying for nearly three months with nothing more elaborate than a moist piece of decaying wood clamped lightly to an ordinary microscopic slide and kept in a moist, dark box. Many of the larvæ were content with conditions on the surface of the wood, next the glass, and were therefore easily observed. These remarkable larvæ are very hardy; prolonged dryness simply results in a suspension of activities, while they are quite resistant to an abundance of moisture. Embryos will continue their development in mother larvæ even

though the latter be kept for weeks in a sealed cell filled with water. It is even possible to make very satisfactory photomicrographs of living embryos within the mother larvæ, while there is not the slightest difficulty in observing the movements of those nearly fully developed before their escape from the mother integument. With our present knowledge we see no reason why artificial colonies of this insect might not be established in the vicinity of a zoological laboratory and maintained with very little or no attention from year to year, if not for a decade or more. A detailed account of this species, with a number of illustrations will appear shortly in the writer's report for last year. E. P. FELT

TREMATODES OF THE DRY TORTUGAS

MY friend Albert Hassall has called my attention to two of the new generic names which I employed in a recent paper on the Trematodes of the Dry Tortugas.[1]

The generic names *Didymorchis* and *Mesorchis* are preempted, thus making it necessary to invent other names to take their place. I therefore propose for *Didymorchis* the name *Pycnadena* (πυκνὸς packed close, and ἀδὴν a gland), and for *Mesorchis* the name *Antorchis* (ἀντίος opposite, and ὄρχις).

These two specific names hence become *Pycnadena* (n. g.) *lata* (Lt.) and *Antorchis* (n. g.) *urna* (Lt.).

At the suggestion of Mr. Hassall I take advantage of this opportunity to state that *Deradena ovalis, Hamacreadium mutabile* and *Genolopa ampullacea* are the type species of their respective genera.

EDWIN LINTON
WASHINGTON AND JEFFERSON COLLEGE,
WASHINGTON, PA.,
January 23, 1911

QUOTATIONS

COMMERCIALISM IN EDUCATION

THAT the methods of higher education are in a state of transition in this country appears evident from the pedagogic innovations made and the many more proposed during the past several years. On the one hand, colleges of higher standing are elevating their curricula to a real professional plane, apparently having at length found it too difficult, to say the least, to instruct in the same course men for such different callings as dynamo tender and consulting engineer. On the other hand, some schools are frankly revealing that their aim is to serve, not the interests of the student, but solely those of the employer of technical graduates, even though education directed primarily to this narrow purpose may unfit the subject for obtaining the most out of life spiritually, through lowering his ideals and curbing his ambitions, and financially, through making him a mere serf to an industry. A concrete illustration of the haphazard condition of thought concerning technical education is afforded by a recent report of the Carnegie Foundation for the Advancement of Teaching on "Academic and Industrial Efficiency," prepared by a well-known and doubtless capable "efficiency engineer," whose business is concerned with the economical administration of manufacturing establishments.

While we have no quarrel with the efficiency engineer as such, nor with his efforts in the course of business to report upon any problem which his clients may desire investigated, we can not refrain from expressing astonishment at the frame of mind of one who would direct the application to instruction in science of the canons appropriate for running a purely money-making business. The possibility of such a distortion of view is the most serious criticism that could possibly be launched against American educational methods. As well put a skilful and successful sausage maker at the task of criticizing the manufacture of astronomical telescopes. An institution for training young men in science, whether pure or applied, is not a money-getting concern. Its product is not sausages, but the advancement of human intelligence, which may or may not be applied to gainful objects. Even in the narrowest technical sense it does

[1] "Helminth Fauna of the Dry Tortugas, II., Trematodes," Papers of the Tortugas Laboratory, Carnegie Institution of Washington, Vol. IV., pp. 11–98, plates 1–28; issued December 16, 1910.

not turn out a standardized product of salable packages of information, but an infinitely variable and intangible thing the importance of which to the world can not be measured by the demand for it reckoned in dollars and cents. The world ultimately owes far more to the institution which produces men who guide the world's destinies in any department of activity than it does to the graduate factory that adds yet more to the rank and file of the mediocre. This at least is the situation regarding the very class of institutions the investigation of which was undertaken in the report before us. Its whole tenor was to lay emphasis upon the destruction of the academic freedom and initiative that is necessary to the advancement of human intelligence and to promote that kind of organization which, under the disguise of uniformity and system, effectively suppresses progress. It is an application to educational institutions of the methods too common in American manufacture, which insure a large output of the tolerable rather than a small output of the desirable.—*The Electrical World and Engineer.*

SCIENTIFIC BOOKS

Théorie physico-chimique de la vie et générations spontanées. Par STÉPHAN LEDUC, professeur à l'école de medicine de Nantes. Paris, A. Poinat. 1910.

Life as a physico-chemical process, and the analogy between living and lifeless, would possibly have been a better heading for this little book of Professor Leduc, for it does not consider spontaneous generation in the fashion which the reader is apt to expect from its title. On the other hand, it may well be that the post-Pasteurean biologist is over-sensitive as to the words "spontaneous generation," and he is apt to give them in latter days a cabalistic meaning: he inclines to dismiss the rare papers which deal with the theme as anachronisms—and he is careful not to recommend them to publishers. Even the French Academy has become so modern that it will not admit to its shelves any treatise which deals with this "exploded theory"!

Nevertheless, a whisper comes occasionally out of the wilderness and reminds us that this is the problem of all biological problems and that it is still neglected. Perhaps our conscience is touched by the feeling that if we are consistent evolutionists we must have some manner of faith that the living came from the lifeless, and that, in the pursuit of biological happiness, we have been drifting in past years towards vitalism in some type or another. We recall too, that in the last decade, steps have been taken in the analysis of biochemical phenomena, in matters of enzymes, catalyzers, ions, tonicity and similar physical facts which have all an intimate bearing upon organisms and are paving the way for a new biological era. After all, many of us are convinced mechanists, and there should therefore be no reason why a book like Leduc's, title and all, should not be welcomed. It is certainly the first work to bring up to date the documents upon which a synthetic biology—as distinguished from descriptive and analytical—may be founded.

Let us see how his theme is handled: There is as yet no satisfactory definition of life: in spite of the efforts of many biologists, we know it by its presence or absence, by phenomena of nutrition, sensibility, growth, organization, reproduction, processes all of which are known in some degree in the inorganic. Moreover, as Leduc declares, life is in itself different in quality in its different manifestations in various organisms; thus he leads us to infer that the life of man differs more widely from the life of a protozoan than the life of the protozoan from the "life" of liquid crystals, for example. Life is to be studied as the transformer of matter and energy, it is a specialized phase of matter, the organic as opposed to the inorganic, and like a current it changes ephemerally. Its expression can best be studied in nutrition and in morphogenesis. And these are the lines of study which the author has developed. Nutrition is in essence chemico-physical, especially concerned with the phenomena of contacts between fluids of different characters, whether electrolytic, osmotic, colloidal, crystalloidal. In this connection he considers the laws of solutions,

matters of molecular concentration, osmotic tension, cryoscopy, periodicity, Ostwald's surface energy, relations to thermodynamics, diffusion, fields of force, all considered as elementary factors which enter into the synthesis of organic phenomena. In the matter of diffusion Leduc's experiments show curiously close parallels with organic processes, producing geometrical forms, circulation of "cytoplasm," "life" which survives freezing or drying, pseudosegmentation of a "germ," and phantoms of karyokinetic figures. Especially striking are osmotic growths, which are unquestionably the most complete parallels between the lifeless and the organic which have been devised. Following Leduc's formulæ one may cause "organisms" to grow which are curiously like algæ or fungi. Leduc points out that they will grow roots, stems and "fruit," the last sometimes appearing quite different in color. The growths have their periods of "youth, maturity, senility and death," they exhibit periods of activity and rest, they show cell-like divisions, definite form relations, and a circulation of their fluid contents; they will repair wounds, and show responses to external and internal stimuli. Peculiarly sensitive are these colloidal osmotic productions to changes in milieu: thus those "growing" around the sides of a jar will occasionally behave differently from those in the middle. Each salt, it appears, has its specific morphogenic properties. With some salts the "productions" are first attached: they then become amœboid and motile, sometimes forming spicules at the surface. The degree of concentration of the solution, also, determines sensitively the branching or the heaviness of the growth, the outcome of reactions which Leduc compares, by numerous chemical formulæ, to metabolism.

Altogether Leduc's book is interesting and it deserves to be carefully read. We need not admit that it *is* biology; but we must admit that the inorganic conditions which here are given detailed consideration have occurred and are occurring constantly in organisms. And we shall be apt to admit that the synthetic method promises results which will prove of great value. Leduc would be the first to agree that living substance may not be synthetized for ages, if at all. But each advance brings the goal nearer, in the solution of single problems, and even of their separate components. Leduc points out the immediate task of synthetists, and an essential one, is not the artificial production of albumenoids, but of a chlorophyllian substance which will decompose carbon-dioxide dissolved in water and be capable of assimilating carbon. In this direction one recalls the interesting notes of Matthews and of McPherson in recent numbers of SCIENCE.

BASHFORD DEAN

Praktikum der Bakteriologie und Protozoologie. Von KISSKALT und HARTMANN. Zweite, erweiterte Auflage. Zweiter Teil: Protozoologie. Von Dr. M. HARTMANN. Pp. vi + 106. Mit 76 teils mehrfarbigen Abbildungen im Text. Jena, Gustav Fischer. 1910. M. 4.

The task of securing adequate laboratory material for instruction in protozoology has been considerably simplified by the "Praktikum" of Kisskalt and Hartmann. The author of the second part, Dr. Hartmann, is the director of the laboratory of protozoology in the Royal Institute for Infectious Diseases at Berlin, a pupil of Professor R. Hertwig and the successor of Schaudinn as editor of the *Archiv für Protistenkunde.* The work is therefore authoritative and reflects the current practise in one of the greatest centers of research. The hand-book is written primarily for the medical student and includes only parasitic forms and especially those of medical interest. It is not a book therefore primarily for the biological laboratory though the range of forms it discusses is sufficiently wide to make the work indispensable to every student of the protozoa, and of greatest value in all laboratories in which the protozoa are studied.

The second edition has been considerably enlarged by the addition of a section on the technique of investigation in protozoology and by a chapter on the Myxosporidia and the Sarcosporidia. A number of new parasitic

types, principally from culture animals, have been added, including *Amœba lacertæ, A. diploidea, Entamœba muris, E. tetragena, Trichomonas muris, Lamblia* (and *Octomitus*) *muris, Leucocytozoon ziemanni, Proteosoma præcox* and *Balantidium coli.*

The plan of treatment is comprehensive, including general introductions to each group, and detailed accounts of the morphology and life history of each of the forms discussed together with directions for securing, controlling, cultivating and preparing the material for study. Illustrations often in color, illustrate the various stages and assist materially in the interpretation of laboratory material. Brief bibliographies of a few pertinent papers are appended.

It is to be regretted that the student of *Babesia canis* is left uninformed of Nuttall's work, that Fantham's work as well as Schellack's on spirochætes is not cited and that the sexual phase of the cycle of *Trypanosoma lewisi* is described as reported by Prowazek in *Hæmatopinus* without any hint as to the reserve with which his conclusions on this point have been generally received. This lack of caution is all the more regrettable in the light of Minchins's experiments with fleas as carriers and Doflein's recently published results of his experimental cultures and his conclusions as to the necessity of caution in interpreting stout and slender forms as sexual gametes and their conjunction as conjugation.

No chapter on technique of parasite flagellates is complete either historically or technically without calling the student's attention to the culture methods of Novy and MacNeal. Such omissions as these are hardly to be condoned by the fact that the author is writing primarily for the German student.

The figures are often original and are uniformly excellent. The condensed but comprehensive and lucid account of the significant features of the structure and life history of the important pathogenic and parasitic types available for laboratory use will be of greatest assistance to the student in this difficult field.

Charles A. Kofoid

Berkeley, Cal.

TRIPHENYLMETHYL

Since modern methods of formulation were established, organic chemists have been able to represent the many thousands of compounds, whose constitution has been elucidated, by formulas in which the carbon atoms are always tetravalent. The single exception was carbon monoxide, CO, in which the carbon is necessarily represented as being bivalent.

In view of these facts it was natural that Gomberg's discovery of "triphenylmethyl," $(C_6H_5)_3\overset{*}{C}$, should arouse widespread interest, because, if it be correctly formulated, the carbon atom marked * is trivalent. During the ten years which have elapsed since Gomberg's discovery was first announced, a very large amount of work has been carried out in order to elucidate the true nature of triphenylmethyl. The most important contributions, which are summarized below, have been made by Gomberg himself, by A. E. Tschitschibabin, A. von Baeyer and more recently by W. Schlenk[1] and his co-workers in Baeyer's laboratory.

"Triphenylmethyl" is prepared by the action of certain metals, such as zinc, on triphenylchloromethane, $(C_6H_5)_3CCl$; the metal simply removes the chlorine atom. "Triphenylmethyl" exists in two forms, a white, solid modification, which is relatively stable, and a soluble yellow form exhibiting very great chemical activity. This colored variety has a molecular weight corresponding to the simpler formula, $(C_6H_5)_3C$.

As regards the colorless material, the facts pointed to its being hexaphenylethane, $(C_6H_5)_3C\cdot C(C_6H_5)_3$, but many chemists hesitated to accept this view, chiefly, perhaps, for the following reasons. The substitution of phenyl groups for hydrogen in hydrocarbons results, in general, in an increase in the stability of the product, consequently, in passing from ethane, $CH_3\cdot CH_3$, to hexaphenylethane, $(C_6H_5)_3C\cdot C(C_6H_5)_3$, we should expect to obtain an inert substance, but we find that the "hexaphenylethane," mentioned above, is so unstable that its mere solution, at the ordi-

[1] *Ber. d. chem. Ges.*, **43**, 1753, 3541, 1910.

nary temperature, causes its decomposition into "triphenylmethyl," $(C_6H_5)_3C$.

An investigation of the ethanes containing a smaller number of phenyl groups, from one up to five, would obviously be calculated to throw light on these points. This work has been carried out and it has been found that a very stable compound, which was formerly regarded as being hexaphenylethane, actually possesses a different constitution. It has also been shown that, in certain respects, there is a decrease in stability and an increase in chemical reactivity as the number of phenyl groups in the ethane molecule becomes greater. Thus, for example, pentaphenylethane, $(C_6H_5)_3C \cdot CH(C_6H_5)_2$ is decidedly less stable than tetraphenylethane, $(C_6H_5)_2CH \cdot CH(C_6H_5)_2$.

The final link in the chain of proof has been furnished by Schlenk, who has just shown that if the pentaphenylethane be heated with a neutral solvent of high boiling point, it is decomposed into triphenylmethyl and tetraphenylethane, in the manner indicated by the dotted line in the formula,

$$(C_6H_5)_3C \; CH(C_6H_5)_2;$$

the tetraphenylethane results, of course, from the combination of two of the groups, $CH(C_6H_5)_2$. It follows, therefore, that there is no difference, in principle, between the behavior of pentaphenylethane and hexaphenylethane towards solvents; when in solution, both give triphenylmethyl, the latter at the ordinary temperature, the former only when it is heated.

In view of these results there is no ground for doubting that the colorless solid obtained by Gomberg is really hexaphenylethane, $(C_6H_5)_3C \cdot C(C_6H_5)_3$, and that its passage into solution, at the ordinary temperature, suffices to resolve it into two molecules of colored triphenylmethyl, $(C_6H_5)_3C$.

J. BISHOP TINGLE

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SPECIAL ARTICLES

SUGGESTIONS AS TO THE CULTURE OF BUTTERFLIES

BUTTERFLIES, with their clear-cut color patterns and brilliant hues, their remarkable polymorphism seen in the occurrence within a single species of two or more seasonal forms, or of melanic, albinic or other varieties often limited to individuals of one sex, furnish a most inviting field for the student of evolution and heredity. The fact that in America, at least, no precise and long-continued work on heredity in diurnal lepidoptera has been undertaken hitherto is probably due in part to the belief that the mating of butterflies, occurring usually in the air, would be difficult to bring about in small cages; though the mating of moths under such conditions is generally known to be an easy matter. It is my purpose in this article to correct this false impression in regard to the mating of butterflies, and to make other suggestions as to methods of caring for this dainty live stock, of marking individuals to indicate their pedigree, and of preserving them in a more compact, permanent, convenient fashion than the usual impalement on long pins in bulky drawers or boxes.

One who undertakes the study of the heredity of butterflies may of course begin either with live wild females, presumably already impregnated, or with eggs already laid, with larvæ, or with chrysalids. For transportation over long distances chrysalids, or eggs upon the food plant, are usually to be prefered. Living plants with roots intact and leaves covered with eggs may be shipped in tin boxes by mail. But often only the imago can be obtained. In this case, and always, if the distance is not too great, sending live butterflies by mail in strong, cylindrical tin boxes lined with moist blotting paper that is held firmly in position is to be recommended. I have found that *Colias philodice,* shipped 150 miles in this way, and shut up closely for eighteen hours, stands the journey well, and lays abundantly, if well fed with sweetened water upon arrival.

For purposes of identification it is of course necessary for the student of heredity to label each living adult butterfly. This is readily done by writing the sign of the family and individual with pen and ink upon the under

surface of the hind wings. Since aqueous ink will not flow readily enough, the pigment used should be dissolved in 50 per cent. alcohol, and applied with light strokes of a stub pen. Anilin dyes are convenient, as they are readily soluble and can be obtained in almost any color, but for general use any carbon ink that is not precipitated by mixing with 50 per cent. alcohol is preferable, because more permanent. The live butterfly, held firmly with smooth-tipped entomologists' forceps clasped across the fore and hind wings close to the body, objects very little to light strokes of the pen, and the alcoholic ink dries quickly.

For the designation of a family from a single mating, that is, the progeny of one female, I use a small letter, reserving the corresponding large letter for the original wild female. The individuals of each family as they pupate, or, if more convenient, as they emerge from the chrysalis, are marked with arabic numerals written at the right of the family letter, like an exponent. In butterflies of course these numbers may run up to over 100. In one family of *Colias philodice* that I raised during the season of 1910 there were 123 brothers and sisters. Suppose this family were the offspring of the wild female, "A," and were called "a." The last to pupate (or to emerge) would be "a^{123}." To facilitate the handling and recording of large families, it is sometimes well to raise the successive batches of eggs laid by a single female separately, designating each successive lot with a numeral written as a coefficient, so that $2n^{10}$ would mean the tenth butterfly to pupate (or to emerge) in the second lot of eggs laid by the mother of family "*n*." The name and pedigree of this female, and that of her mate, would be recorded, of course, at the head of the sheets on which the dates of pupation and eclosion of their offspring are set down.

No elaborate outdoor quarters are needed for keeping and mating live butterflies. The air of the laboratory needs only to be fresh and fairly moist, as that of the ground-floor rooms of large buildings of brick or stone is likely to be in summer. The parched air of a steam-heated room, or of one upon an upper floor and flooded with direct sunlight, is more fatal to butterflies than complete absence of food in a moist atmosphere. Bottomless cages 15 inches in length and breadth and 10 inches high, consisting of a simple frame of pine strips covered inwardly with cotton mosquito netting, are of ample size for *Colias philodice*, serving as a vivarium for the pupæ, as an enclosure for mating, and as a cage for the female during egg-laying, if the food plant is small enough to be covered by a frame of this size. The use of wire screening is not to be recommended for adult butterflies, as it soon wears out and disfigures the wings that beat upon it. A frame of this sort covered with cheese cloth, or better with the material known by milliners as frame covering, makes an excellent breeding cage for even the youngest larvæ.

The imago that has just crawled up from its chrysalis rests until the blood flows from the abdomen into the tiny pupal wings. In *Colias philodice* this requires about five minutes, but the wings after reaching full size remain limp for about a half hour; and marking should be deferred until they harden. Then the males of each family may be placed together in one cage, the females in another. On the day following eclosion they will be ready to feed, and bouquets moistened with a solution of honey and water, or brown sugar, should be placed in the cages.

It is surprising to a beginner to see how readily live butterflies may be handled in the absence of direct sunlight or intense diffuse light. Even out of doors, after sunset or in the early morning, they may be allowed to creep upon the finger wet with sweetened water, and feed. They are attracted by a warm moist hand, as by a flower, and on holding them by the costal margin of the wings with one hand, and allowing them barely to touch with the feet a moist finger or palm of the other, they may be stimulated to extend the tongue and begin feeding. Once feeding has begun, they may be moved to a generous drop of honey and water and allowed to drink their fill. In dull, cold weather butterflies neither feed nor lay, and it is necessary in midsummer, when metabolism is rapid, that

individuals to be used in breeding should have at least one square meal a day, served either in the early morning or evening, when they can be handled without danger of escape. In case they are actively flying during the day, and visit the moistened bouquet provided for them, they will of course feed themselves, but they are more strongly attracted toward the source of light than toward food, so that, if only slightly active, they may not reach the food supply at all. This is one reason against the use of large cages. They do not go in search of food, as the bee seems to do, but, stimulated by light to activity, they find it almost by accident. Hence they must be kept near the food.

The same reason applies to the use of small cages for mating. The larger the cage, the smaller are the chances that two individuals will meet. I began my experiments by turning butterflies loose in a large screened veranda, strongly lighted only on one side. Under such conditions their attraction towards the light absolutely controlled them. Each went his or her own way, paying not the slightest attention to the others. If several males and females of the same species are placed in a cage of the dimensions noted above (10 inches high and 15 inches square) or 15 inches in all three dimensions, and kept in the direct sunlight, or, if the temperature is high enough, in strong diffuse light, some matings may be expected. As soon as a couple are mated they should be removed to a separate cage, and their numbers noted. Mating continues in *Colias philodice* for over an hour, usually for about an hour and a half, and often two or three hours, so that there is little danger of promiscuity when large numbers of both sexes are placed in the same cage, if properly watched. One male often can be mated on successive days with several females.

Fertilized females often begin to lay on the day after mating. They will lay quite well indoors under warm, sunny conditions, provided they are well-fed, but in any case the food plant on which they lay should be growing, so that the minute larvæ, upon hatching, will find fresh food awaiting them. The most convenient way to obtain the eggs and rear the larvæ of *Colias philodice,* for example, is to place the butterfly in a cage over healthy clover growing on a lawn that is free from ants and slugs.

The food plants and feeding habits of larvæ are so varied that few general directions can be given for their care. Caterpillars that feed on coarse leaves that wilt slowly when gathered may be kept in large, flat, cloth boxes, like those that are used at the Gypsy and Brown-tail Moth Laboratory at Melrose Highlands, Mass. But the leaves of most food plants of butterflies soon become wilted and dry under such conditions, and it is preferable to enclose the growing plant in a cage, or a branch of the shrub or tree in a bag of cheese cloth. If kept in close glass vessels to prevent evaporation, or even in large moist vivaria, there is danger in *Colias philodice,* at least, of infection with an intestinal bacterial disease that may kill a large proportion of both larvæ and pupæ. So a good general rule is to keep the larvæ upon their food plant out-of-doors, and well screened from insect parasites and birds.

Pupæ should be carefully guarded from mice and slugs, and if either are at all abundant the chrysalids should be taken indoors. *Limax maximus* will gnaw through a fine cloth netting on the inside of which a pupa rests, and eat the chrysalis, leaving a small hole through the screen, the margin of which is blackened with the salivary juices of the slug, which apparently contain sulphuric acid.

The student of the genetics of butterflies will hardly be content with the ordinary museum methods of mounting specimens upon pins stuck into cork. Greater compactness, perfect safety from museum pests, and quick accessibility to both surfaces of the wings are needed. The patent Denton mounts for individual specimens satisfy these requirements well, but they are expensive for use in large numbers and, though vastly superior to the ordinary pin-cushion method, are not convenient to handle when huge families are being examined and compared, because each specimen must be picked up separately. All these needs, however, are met by a simple form of

case which the present writer has used with much satisfaction.

The mount consists of a rectangular, wooden frame of any desired size (*e. g.*, 10 × 12 inches) made of pine strips three eighths of an inch square in thickness, mitered or mortised at the corners, holding apart two sheets of glass corresponding in size to the outer edge of the frame, one for the top and one for the bottom of the mount, which is bound together with passe-partout 1¼ inch wide. As for the glass, old photographic plates, 10 × 12 inches in size, cleaned with caustic potash solution, are convenient in dimensions, thin and light, and of good quality.

Each butterfly is prepared by stretching and drying it upon its dorsal surface, pinning it temporarily until it has been made fast with strips of paper. The wings of the dried specimen must lie flat, or be inclined slightly ventrad, but never dorsad. It is then fastened, with a small drop of thin liquid glue applied to the dorsal surface of the thorax, to the sheet of glass that is used as the upper pane or cover. Small strips of sheet lead (about 1 inch × 1½ inches), bent into an arch, make a convenient weight to set astride the wings until the specimen is well fastened to the glass. The pane is then inverted over the frame, and glued to it. The lower sheet should not be glued to the frame, but fastened to it only by the strip of passe-partout, 1¼ inch wide, which is used to hold the two panes of glass together and seal the mount. If a specimen should get loose, the bottom glass may be easily cut away, repairs made, and the case sealed with fresh passepartout. If the upper pane should get broken, it is a simple matter to remove the specimens, using steam when necessary, and remount them. Care should be taken in the preliminary stretching of the specimens lest the feet should project more than necessary, so that, when the case is put together, they come into contact with the lower plate, and loosen the attachment to the upper. For ordinary purposes, however, it is only necessary to trim off the tips of a few that project excessively.

Seven hundred butterflies of the size of *Colias philodice* can be filed away in the space of a single cubic foot, in the mounting frames just described, each case measuring 10 × 12 inches in breadth and length and one half inch in thickness, and containing 25 specimens. They are sealed from dust and vermin, and easily examined on both surfaces in groups of convenient size.

A rich field for conquest awaits any one who chooses to leave the beaten tracks of entomology and scout among the fastnesses of experimental evolution. When one considers the remarkable results that have been accomplished single handed by such observers as Standfuss, Tower, Doncaster and T. H. Morgan, not to mention many others, the possibilities achieved in this field if the huge army of observers already interested in insects should attack in an organized way the problems of variation, the inheritance of acquired characters, mutation and natural selection, polymorphism and sex, mimicry and protective resemblance, can hardly be overestimated. Desultory observations of the strolling naturalist will not help much in this conquest, but long-continued breeding of carefully selected strains under well-controlled conditions can not fail to win valuable results.

Entomological societies and journals of the future, in order to contribute effectively to the real advancement of science should organize cooperative plans of research along these lines, and enlist the services of the countless observers whose random notes now fill their archives.

John H. Gerould

Variegation of European Alfalfas

As a part of the extensive investigations being conducted with alfalfa at the Dickinson, North Dakota Sub-station, a series of European alfalfas was planted in the nursery in 1909. A study of some of the plants in 1910 revealed the presence of variegation in flower coloring. This was expected to a certain extent. As a preliminary to the determination of the correlation of the variegation to other characters, both physiological and morphological, the percentage of variegation was determined for each strain or planting.

Variegation is a feature of certain alfalfas, which undoubtedly indicates that at some previous time at least one parent has been the yellow-flowered sickle lucern, *Medicago falcata*. The flowers of the pure *Medicago sativa* retain their original color from the time of blooming to the time of withering. The color may range from nearly white to some shade of violet, but the color, whatever it may be, remains stable during the period of bloom. The flowers of *M. falcata* are of a chrome yellow, and remain constant, as do the flowers of *M. sativa*. The flowers of the hybrids of these two plants show a range of coloration during bloom, before withering, which, in extreme cases, runs from rich pansy violet through lettuce-green to coppery-yellow. Even in plants having apparently but a small proportion of *falcata* parentage this variegation of color is retained, though in a manner much less pronounced than in the case instanced.

The alfalfas under experiment which are given below were secured through the United States Department of Agriculture. Many of them were obtained from abroad through the energy of Mr. Charles J. Brand, of that department.

The number at the left of each strain is the seed and plant introduction number of the Department of Agriculture. The names are the translations of the German common names, or in many instances, the locality whence the seed was obtained is alone given. The total number of plants of each strain and the percentage of variegation are given.

A supplementary table is given which groups the alfalfa strains according to their geographic origin or to the name borne by them in Europe.

Summarizing the foregoing table we have the following:

[1] At the time of determination of variegation the various rows were indicated by arbitrary numbers. The results were thus not biased by previous knowledge. The variegation was determined by the presence or absence of color change in the standards of the unwithered flowers. This was readily determined in most cases. But in any case, all plants were subjected to a practically uniform judgment.

S. P. I. No.	Name and Source	Number of Plants	Per[1] Cent. Variegated
25110	Commercial Sand Lucern (Switzerland)	22	72.7
25178	Commercial Sand Lucern (Bohemia)	39	64.2
25175	Old German Franconian alfalfa (Bavaria, Germany)	43	56.0
24667	Old German Franconian alfalfa (Bavaria, Germany)	46	54.4
25182	Eifeler lucern (Rhenish, Prussia)	13	54.0
25267	German alfalfa (Prussia)	35	51.5
24733	Old German Franconian alfalfa (Baden, Germany)	43	49.0
25184	Provence alfalfa (Germany)	15	46.6
24602	Provence alfalfa (Germany)	42	40.5
25194	Old German Franconian alfalfa (Bavaria, Germany)	32	40.5
25257	Pfalzer lucern (Baden, Germany)	28	39.4
24923	Old German Franconian alfalfa (Württemberg, Germany)	42	38.0
25269	Roumanian alfalfa (Southern)	36	36.0
24603	Commercial Sand Lucern (Erfurt, Germany)	42	35.8
24635	Old German Franconian alfalfa (Baden, Germany)	41	34.2
24740	Italian alfalfa (northern Italy)	42	33.4
25022	Old German Franconian alfalfa (Baden, Germany)	43	32.6
24721	Provence alfalfa (France)	35	31.5
24767	Old German Franconian alfalfa (Baden, Germany)	42	31.0
25183	Old German Franconian alfalfa (Baden, Germany)	13	30.4
25115	Commercial Sand Lucern (Bromberg, Prussia)	37	29.8
25091	Commercial Sand Lucern (Strasburg, Germany)	41	29.2
24727	German alfalfa (Baden)	44	27.3
24741	Commercial Sand Lucern (Bohemia)	44	27.3
24735	Italian alfalfa	38	26.2
23394	Commercial Sand Lucern (France)	34	26.2
25112	Commercial Sand Lucern (Switzerland)	24	25.0
24718	Moravian alfalfa (Bohemia)	43	23.3
25193	Old German Franconian alfalfa (Baden, Germany)	39	23.0
24728	German alfalfa (Baden)	44	22.7
25111	Commercial Sand Lucern (Switzerland)	22	22.3
24722	Provence alfalfa (France)	45	22.2
25268	Commercial Sand Lucern (Bohemia)	37	21.4
25176	Commercial Sand Lucern (Bohemia)	43	21.0
23481	Commercial Sand Lucern (Hamburg, Germany)	40	20.0
25167	German alfalfa (Thuringia)	41	19.5
25270	Roumanian alfalfa (northern)	36	19.4
24719	Hungarian alfalfa (Austria)	43	18.6
24717	Bohemian alfalfa (Austria)	44	18.3
24730	Russian alfalfa (southern Russia)	46	17.4

S. P. I. No.	Name and Source	Number of Plants	Per Cent. Variegated.
25187	Italian alfalfa (Pisa)	36	16.6
24928	Provence alfalfa (Germany)	39	15.3
24723	Russian alfalfa (southern Russia)	46	15.2
24858	Italian alfalfa (Florence)	40	15.0
25181	Pfalzer Lucern (Bavarian Palatinate, Germany)	14	14.3
24732	Russian alfalfa (northern Russia)	46	13.0
24720	Provence alfalfa (Germany)	39	12.8
24729	Hungarian alfalfa (Austria)	44	12.7
23396	Commercial Sand Lucern (Darmstadt, Germany)	35	11.4
24731	Russian alfalfa (southern Russia)	45	11.1
25186	Algerian alfalfa (Setif, Algeria)	19	10.5
25180	Moravian alfalfa (Bohemia)	39	10.3
24724	Russian alfalfa (northern Russia)	41	9.7
24737	Commercial Sand Lucern (Bohemia)	41	9.7
24734	Provence alfalfa (Germany)	42	9.5
25185	Hungarian alfalfa (Austria)	13	7.7
25179	Hungarian alfalfa (Austria)	40	7.5
24725	Spanish alfalfa	33	6.0
25109	Austrian alfalfa (Vienna)	37	5.4
25168	Commercial Sand Lucern (Bohemia)	40	0
24736	Spanish alfalfa	36	0
24726	Turkestan alfalfa	43	0
24738	Turkestan alfalfa	42	0
24739	Turkestan alfalfa	43	0
	Average	37	24.1

Name	Number of Strains	Average Per Cent. of Variegation	Average Deviation
Turkestan alfalfa	3	0	0
Spanish alfalfa	2	3	3
Austrian alfalfa	1	5.4	0
Algerian alfalfa	1	10.5	0
Hungarian alfalfa	4	11.6	4.0
Russian alfalfa	5	13.3	2.4
Moravian alfalfa	2	16.8	6.5
Bohemian alfalfa	1	18.3	0
Italian alfalfa	4	22.8	7
Provence alfalfa	7	25.5	12
Palatine (Pfalzer) alfalfa	2	26.8	12.5
Roumanian alfalfa	2	27.7	8.3
Commercial Sand Lucern	15	27.7	12.4
German alfalfa	4	30.2	10.7
Old German Franconian alfalfa	10	38.9	8.9
Eifel alfalfa	1	54.0	0
Combined variegated forms (*i. e.*, omitting Turkestans)	61	25.3	

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

At the 234th meeting of the society, held on Wednesday, November 30, 1910, the following papers were read:

Regular Program

The Influence of Marine Currents on Deposition in Continental Seas: E. O. Ulrich.

This paper tends to prove that Mr. Bailey Willis's views in regard to non-deposition in continental seas as the result of current action are in the main unfounded. In brief, Mr. Willis's views are that the numerous minor hiatuses in the geologic column are commonly to be attributed to non-deposition and even to submarine scour, resulting from marine currents, rather than to emergence of the sea bottom. In preface brief allusions were made to instances of local thinning or absence of sediments that may be justly ascribed to current work. It is doubtless true that marine currents flow at certain times through sub-marginal troughs like the Lævis channel.

Arguments were brought against Willis's views under two headings, namely: the improbability of the existence in Paleozoic continental seas of currents competent to bring about such results; and the lack of evidence of such action having taken place under conditions obviously the most favorable for the existence of such currents.

Currents of sufficient intensity to cause an appreciable interruption of deposition over wide interior areas could only exist in great seas, in which the admittedly necessary "trans-continental currents" of Willis might be developed. Such seas have no foundation in fact. At any given time the Paleozoic seas of North America were far less extensive than those delineated by Willis or even those depicted in Schuchert's "Paleogeography of North America." Such maps are synthetic, giving the maximum development of several successive seas. The Black River—early Trenton submergence—having, as generally believed, the greatest areal development of any Paleozoic seas, may be taken as the extreme example. This submergence consisted of no less than five and possibly six distinct transgressions, as shown by the areal distribution of the successive faunas and of the beds containing them. These six faunas are sharply defined and any two juxtaposed faunas show clearly by the varying direction of the overlap of their containing formations that they invaded from quite different oceanic basins. Moreover, no two of these faunas

could have been synchronous, since there is no evidence of intermingling of species characterizing the respective formations in the median areas of interfingering overlaps.

It is only the pelagic and semi-pelagic types that can be depended upon for exact correlation between widely separated areas, and prove the existence of unobstructed current-highways. Referring to such organisms, the distribution of the Eopaleozoic graptolites offers very strong arguments against the hypothesis of transcontinental currents in the interior basins. The most important of the graptolite faunas are confined to current-swept submarginal channels. Had these currents continued across the continental border, as assumed by Willis, the graptolites must have been carried by them into and through the interior seas, a condition wholly negatived by the evidence in hand.

Perhaps the strongest argument against the efficiency of currents in preventing deposition in the interior continental seas is found in stratigraphic overlaps. In any period of sea-advance, beds are deposited by overlap toward the "positive" or relatively elevated areas. At the same time, the submergence increasingly favors the formation of currents. If currents were present and competent to cause scour, phenomena quite opposed to those observed would obtain. The lower beds, which are absent, should be present, and the later beds, formed in a sea supposedly favorable to the extensive development of strong currents, should not be deposited, or should show the effects of current action by diminished thickness.

Notes on Argentina: BAILEY WILLIS.

No abstract. EDSON S. BASTIN,
Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 235th meeting of the society, held on December 14, 1910, Mr. M. R. Campbell, the retiring president, presented an address, entitled "Historical Review of Theories Advanced by American Geologists Regarding the Origin and Accumulation of Oil."

At the close of Mr. Campbell's address the eighteenth annual meeting of the society was held for the purpose of electing officers, and the following officers were elected for the ensuing year:

President—Mr. Alfred H. Brooks.

Vice-presidents—T. W. Stanton and David White.

Treasurer—Hoyt S. Gale.

Secretaries—Edson S. Bastin and Robert Anderson.

Members at Large of the Council—W. C. Mendenhall, Wm. C. Alden, F. C. Schrader, F. B. Van Horn, Adolph Knopf.

FRANÇOIS E. MATTHES,
Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

THE 236th meeting of the society, held on Wednesday evening, December 21, 1910, in the Cosmos Club, was devoted entirely to a discussion of Mr. E. O. Ulrich's paper entitled "The Influence of Marine Currents on Deposition in Continental Seas," delivered before the society on November 30, 1910.

The discussion was opened by Mr. Bailey Willis, who summed up the propositions from which he wished to dissent as follows: (1) that the epicontinental seas of eastern North America during the Middle Ordovician age were so landlocked that marine currents could not have passed through them with sufficient force to have influenced the deposition of sediments, and could not have kept the bottom clean of sediment in any portion of the area which was submerged; (2) that the Gulf Stream does not go to the bottom of its channel and does not scour the bottom. The first, which was brought forward by Mr. Ulrich, he considered largely theoretical, and founded upon an interpretation of the distribution idea which has not sufficient support in the physical evidence of unconformities that must have been developed by subaerial decay and erosion upon exposed portions.

Mr. Willis recognized that there are areas characteristic of the shores of the Ordovician sea and of the shores of islands in that sea where unconformities may be recognized by the usual evidences of erosion; and also that there are other localities where limestones are wanting that are elsewhere developed to notable thicknesses. In many of these partial sequences there is no evidence of exposure to subaerial agencies. In these cases it is reasonable to consider the alternative proposition of marine scour as of at least equal value in interpretation with that of the former existence of land.

In answer to Mr. Ulrich's statement that the Ordovician seas were landlocked, he pointed out the evidence brought forward by paleontologists for the wide distribution of faunas composed of numerous species, and maintained that marine currents were the most effective agencies in promoting that distribution. Were the seas so land-

locked as Mr. Ulrich supposed, the food supply would fail and the condition which has been reached by the present Black Sea whose waters are incapable of supporting life that in any way approaches that of the Ordovician, might be reached.

Referring to the paleogeographic maps which have been issued by Dr. Schuchert and to Mr. Ulrich's views on the details of paleogeography in the Paleozoic age, Mr. Willis held that no one is yet in a position to interpret the evidence for limited intervals of time. The study requires the most searching investigation of the different lines of evidence and a better understanding of the principles that shall govern the interpretation before any more than general outlines of the geography can be mapped. Dr. Schuchert's maps represent the distribution of faunas, and for each fauna which is mapped they represent the extent of the temperature, food and other conditions that determined its habitat. In this respect they are of the highest value. Where, however, the absence of a fauna or a formation has been taken as a proof of the existence of land without evidence of erosion, the maps are misleading, since the alternative hypothesis that the area was submerged, but was inhospitable to that particular fauna, has not received due consideration. The causes which now maintain the great equatorial currents flowing from east to west have been in operation since the oceans were established. Currents and eddies diverted from these main currents by the continental platforms have necessarily been features of all continental seas, and no study of the life conditions of extinct faunas can be adequate that does not take account of the biological and physical effects of such currents. Hence any inference based upon faunas which are interpreted without reference to currents must be fallacious.

In regard to the subject of marine scour, especially by the Gulf Stream, Mr. Willis presented a map showing the submarine deposits of the Caribbean, Gulf and North Atlantic, as given by Agassiz in the "Three Cruises of the *Blake*." That map shows that sediment is deposited under the axis of the Gulf Stream between Cuba and Florida along much of the coast, for the depth of the water is there greater than the depth of the current, which does not reach much below 100 or 150 fathoms, but where the water is shallower or where the current turns from its easterly to a northerly course, it is carried against the bottom and at those points there is a hard limestone bottom covered only by fragments of broken rock and coral, and washed clean by the current. It also appeared from Agassiz's discussion of the action of the dredge that the areas which he mapped as limestone plateaus have a hard and uneven rock bottom, and although the Coast Survey charts over much of the areas so mapped by him show soundings of sand and broken shell, the evidence is that the material brought up on the sounding line is but a superficial coating, locally covering the uneven limestone bottom. Admiral Pillsbury has shown that the volume of water passing a section of the Florida Straits in one hour amounts to nearly 90 billion tons. It has a velocity which varies from two to three and one half knots. The energy of this mass of water could not be lightly checked. Should the sea bottom between Florida and Cuba be gently elevated by an orogenic movement, or should the general level of the ocean be lowered so that the current would reach the bottom, the silt which is now deposited beneath the current would inevitably be swept away. A barrier to the current would only be established in case the orogenic movement raised the bottom more actively than the current could erode it. This could scarcely occur unless the bottom were hard rock. The condition which is thus suggested is that which may be considered as a working hypothesis in explanation of the imbricated limestone of the Ordovician in the eastern United States. It is postulated that there was an extensive sea which was open from south to north, and through which marine currents circulated, as is indicated by the distribution of life. The sea deepened or shallowed from time to time, and there were basins which, according to the great thickness of sediments laid down in them, deepened, while other areas apparently remained as saddles between them. If this interpretation of the physical and faunal facts be correct, it is reasonable to suppose that the currents were at times brought within reach of the bottom and that a condition of non-deposition supervened locally during a more or less prolonged interval.

The discussion was continued by Rear Admiral J. E. Pillsbury, who had command of the Coast Survey steamer *Blake* for five years and spent a large portion of each year investigating the Gulf Stream. He first explained the methods formerly used of gauging marine currents by cans floating on the surface and submerged below, and then the methods adopted by the *Blake* of anchoring the vessel in the stream and measuring the

velocity and direction of the flow on the surface and at various depths by means of a current meter. Many hundreds of observations were taken in the straits of Florida between Fowey Rocks and the Bahamas, between Havana and the Florida reefs, in the Straits of Yucatan, in the Equatorial Current between Tobago and Barbadoes, in all the passages between the West Indian Islands and off Cape Hatteras.

The section off Fowey Rocks was studied during two winter seasons and at other times for brief periods. This is the narrowest part of the Straits of Florida, being about 42 miles in width. On the west side the bottom descends with fairly regular slope to 400 fathoms, at 15 miles distant, while on the east side 400 fathoms is found at but eight miles from the shore. Anchorages were made at approximately seven-mile intervals across the straits. The bottom was found to be branch coral and broken shells at the anchorage nearest the Bahamas, and there was every evidence from the observations that the current reached the bottom here. The stream off Havana flows east, while off Fowey Rocks it has changed its direction 80°, and the inertia of the water in making the turn forces the current to impinge upon the confining bank and carries it to the bottom. Between this point and the western slope the observations showed an average current only to about 300 fathoms depth, while on the western slope itself the current sometimes reached the bottom and sometimes a negative current was observed. At all anchorages except the easternmost, mud was brought up by dredge and frequently on the anchor. At eleven and one half miles east of Fowey Rocks there is an outcropping of rock on which an anchor fouled three times, making it necessary to cut the anchoring rope to get underway, but at other places the bottom was soft. On both sides of the straits the current at times was found to be flowing north as far out as a depth of 10 fathoms or even further off shore.

It has been stated that the presence of gulf seaweeds near Nantucket shows the Gulf Stream to be flowing there. The Gulf Stream is partly caused by the friction of the trade-winds on the surface of the ocean, and partly by the break of the wave which throws a certain amount of water from its crest into the trough. As the trades are persistent, this becomes a simultaneous movement of the whole surface of the sea within trade-wind limits. Gulf seaweed floats with the current, but it is thrown to leeward by the waves faster than any ordinary current can carry it, or when there is no current at all. The water on the Florida reefs about Key West are of milky appearance, while a northerly breeze is blowing which stirs up the bottom coral mud. A southerly breeze brings in the clear water of the Gulf Stream simply by the break of the waves, but no current accompanies it.

The Grenadine Bank extends from Grenada to the Island of St. Vincent and is in the direct line of flow of the trade wind current outside the Caribbean. An anchorage in 17 fathoms near its outside edge showed no current crossing the bank, but the break of the waves was all the time carrying water into the Caribbean across the shoal.

As the discussion was extended to a consideration of the general effects of oceanic currents on deposition, Mr. T. Wayland Vaughan called attention to the investigations Rear Admiral Sir W. J. L. Wharton[1] made around the islands of the Ellice Group and on submarine banks in that region. According to this authority, fine mud and sand may be moved to a depth of 80 fathoms, and there is evidence of the chafing of cables to a depth of 260 fathoms; "volcanic ash can be moved at depths of 30 fathoms or more when exposed to the action of waves in an otherwise deep sea over which strong winds are continually blowing."

Professor J. Stanley Gardiner[2] has described the submarine platform on which the Maldive and Laccadive archipelagoes stand, and has called attention to the generally hard bottom on it. This platform occupies a level about 200 fathoms below sea level, and he says, "there is little doubt but that it is surrounded with precipitous walls or a steep slope for an additional 600 fathoms at least." Professor Gardiner is of the opinion that this platform was formed by marine erosion to the depth of 200 fathoms below sea level. He states, "there is little doubt but that currents may extend to considerable depth and sweep the ocean floor quite bare. Indeed, wherever in the ocean a rocky bottom is found, its character is probably due to an ocean current."

Dr. Paul Bartsch discussed the distribution of the recent marine mollusks with a view to throwing some light upon past conditions. He presented a map showing the extent of the existing faunal areas and called special attention to the

[1] *Nature*, LV., 1897, "Foundations of Coral Atolls."

[2] "The Fauna and Geography of the Maldive and Laccadive Archipelagoes," Vol. 1, Pt. 2, pp. 172, 173.

fact that these areas are coextensive with the existing ocean currents.

He gave some tables, based upon the West American Pyramidellidæ, showing that in this group a remarkably small percentage of the species extended over more than one area. He stated that he considered ocean currents an important factor in the distribution of marine organisms, since they determined practically all the factors entering into the environment in each area, viz., temperature, food, salinity and transportation of larval forms.

Mr. Ulrich, replying to Mr. Willis's discussion, said that most of Mr. Willis's objections had been anticipated and accounted for in the paper read at the previous meeting. It was denied that this paper contained any statement indicating that its author is inclined to the belief that marine currents in the Ordovician epicontinental seas of North America were *never* capable of effecting the deposition of sediments or of keeping the "bottom clean of sediment in any portion of the submerged seas." On the contrary, Mr. Ulrich mentioned a number of instances of locally interrupted deposition attributable to current scour but claimed that these were quite distinct in their causation from the wider discontinuities which have been similarly interpreted by Mr. Willis. It was further denied that the paper sought in any wise to discredit the effect of currents on the distribution of marine organisms. Also that the deduction of frequently shifting, limited and far from transcontinental seas is founded solely on "an interpretation of the distribution of faunas which disregards all the limiting conditions of marine environment except land barriers." Mr. Ulrich insisted that before reaching his conclusions he had considered more or less fully all physical as well as the purely faunal criteria that seemed to have any direct bearing on the problem. Considering that Mr. Willis has paid perhaps as little attention to detailed field investigation of stratigraphic unconformities as to the study of recent or fossil zoology, his short dismissal of conclusions based largely upon such studies as "purely theoretical" seems rather unscientific. His remark respecting detailed paleogeographic mapping, when he said "that no one is yet in a position to interpret the evidence for limited intervals of time" is similarly unjust.

Mr. Willis's statement that the Ordovician seas, "if so land-locked as Mr. Ulrich supposed," would have become, like the Black Sea, "incapable of supporting life that in any way approaches that of the Ordovician," was based upon a misconception of Mr. Ulrich's meaning. As conceived by Mr. Ulrich, Ordovician continental seas invaded the land areas from one or another of the permanent oceanic basins; with which they maintained their connection and from which they derived their faunas throughout their existence.

Evidence of erosion is nearly always to be found wherever a considerable hiatus in the stratigraphic sequence is indicated by the fossils. In view of the fact that such breaks in the stratigraphic sequence commonly extend over by far the greater part of the median area of the continent, it is impossible to account for the wide absence of the deposits and faunas by virtue of any reasonably conceivable current efficiency. Nor can any other interpretation save emergence be advanced to explain the established geographic limitations of the Paleozoic faunas, especially when no fauna of nearly similar age is found in adjacent areas.

The distribution of many types of littoral and bottom-dwelling organisms takes place largely in disregard of marine currents. There are other types, however, notably the plankton and especially reef corals and sessile bryozoa, which propagate by means of free-swimming larvæ, that throw much light on the direction and extent of the currents in the Paleozoic continental seas. In every case where the distribution of the latter has been studied it is found that they rapidly become fewer away from the point of invasion of the particular sea in which they lived. In most instances they disappear entirely before reaching the inner shores of the sea whose extent is determined by continuity of deposits and the presence of other organisms less dependent on currents for their migration. This perfectly competent evidence, therefore, is invariably opposed to Mr. Willis's theory of great inland seas and of transcontinental currents which if present might have been important factors in accounting for stratigraphic hiatuses.

Dr. R. S. Bassler called attention to the fact that the trend of the discussion was losing sight of the stratigraphic side of the question. These distinct northern and southern faunas occurred in distinct formations overlapping to extinction either to the south or north as the case might be. He added that these facts must be explained before currents can be held accountable for the great faunal differences.

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Secretary